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Original Articles.

SHALL PHYSICIANS BECOME SALES-AGENTS FOR PATENT MEDICINES?[1]

BY SOLOMON SOLIS-COHEN, M.D.

SECTION 4 of Article I of the By-Laws of the Philadelphia County Medical Society reads as follows:

"Any physician who shall procure a patent for any instrument of surgery, or who sells or deals in patented remedies or nostrums, or who shall give a certificate in favor of a patented or proprietary remedy or patented instrument, or shall enter into an agreement with an apothecary to receive pecuniary compensation or patronage for sending his prescriptions to that apothecary, shall be disqualified from becoming or remaining a member."

Article VIII, of the By-Laws of the Philadelphia County Medical Society, accepts as an integral portion of those by-laws, the Code of Ethics of the American Medical Association.

A section of that code of ethics treats "Of the Duties of Physicians to Each Other and to the Profession at Large." Article I of that section sets forth "Duties for the Support of Professional Character." Section 3 thereof condemns open or underhand advertising as "derogatory to the dignity of the profession." "These," it says, "are the ordinary practices of empirics, and are highly reprehensible in a regular physician." Section 4 continues:

"Equally derogatory to professional character is it for a physician to hold a patent for any surgical instrument or medicine, or to dispense a secret nostrum, whether it be the composition or exclusive property of himself or others. For if such nostrum be of real efficacy, any concealment regarding it is inconsistent with beneficence and professional liberality, and if mystery alone give it value and importance, such craft implies either disgraceful ignorance or fraudulent avarice. It is also reprehensible for physicians to give certificates attesting the efficacy of patent or secret medicines, or in any way promote the use of them."

To the clearness and force of this dictum nothing can be added. Its wisdom and its justice are beyond dispute.

It is unfortunately true that much of our therapeutics is as yet empirical. Nevertheless the best endeavors of the true physician are directed toward establishing a rational basis for that which experience has proved to be beneficial; toward obtaining scientific data by which to make progress to therapeutics not empirical; and toward eliminating from the traditional heritage of the profession such measures as may have had their origin in superstition or mistaken observation. To use preparations of unknown composition; to use mixtures of innumerable substances, some directly opposed to each other; to use even rational and known combinations of which the ingredients and proportions have not been adjusted to the indications and conditions of the individual case are certainly not scientific methods or practices likely to advance rational therapeutics.

That much of the flavor of the mystery and witchcraft that at one time appertained to the practice of the healing art should have survived among the vulgar, is only a phenomenon to have been expected in the natural course of social evolution. Hence, it is that homœopathy, and mind-cure, and patent medicines have so powerful and so profitable a hold upon the purses of the community, and this not alone among the ignorant and the unlettered. It is a notorious fact that the clergy, presumably educated and intelligent men, are among the most prominent and persistent givers of testimonials to the virtues of advertised nostrums, and medical men and medical journals have long made the religious press a target for satire and invective, because of the hold that the advertising quack has secured upon its columns.

[1] Read before the Philadelphia County Medical Society April 27, 1892. Vice-President Charles H. Thomas in the chair.

In an address to the Medical Society of the State of Pennsylvania, at its last meeting, in calling attention to the alliance between the secular press and the empirics and nostrum-venders, I felt justified in saying that the publishers of magazines and newspapers that allowed themselves to advertise the curative virtues of this or that ready-made preparation or alleged remedial measure, to be applied indiscriminately to all cases—whether of one disease or of many diseases—were accessories to a crime against the unfortunate and the helpless; not alone because of the money filched from the pockets of those deluded by the false promises held out to them; not alone because of suffering unrelieved and lives deprived of their chance for prolongation; but in many instances because of the disease and suffering and death directly produced by the poisonous compounds or noxious gases administered to any that chose to purchase. If such criticism was justified—and who is here that will deny its truth?—if such criticism was justified when applied to those who make no pretence of special knowledge or of devotion to a noble art—to those whose object is solely and avowedly commercial—what language remains to characterize the action of medical journals that permit the insertion in their columns of advertisements such as these that I pass around? What severity of reprobation is adequate for the conduct of the *Journal of the American Medical Association?* What words of condemnation are strong enough for the physician that permits his name to be associated with these devices of the devil?

The frankly-unscrupulous patent-medicine vender, the make of "Safe Cures," or "Temperance Bitters," or "Sure Specifics," is at least to be commended for what, to paraphrase a remark of Senator Benjamin Harrison's, may be termed his "bold brutality." His allegations of philanthropic motives are not intended to be believed; they deceive no one—they are the recognized *ad captandum* devices of the clever advertiser—and, in the sale of his wares, there is no pretence of examination, or of diagnosis, or of prescription based on diagnosis.

Far more iniquitous and far more dangerous to society is the wily manufacturer that advertises "to the profession only." Whether he ostentatiously holds secret the composition of his nostrum, or whether, with pretended frankness, he describes it with an appellative that means nothing, or publishes a formula that cannot be carried out, his object is the same; he seeks to make the physician's the hand whereby he may reach pockets shut from the coarser methods of the Warners, the Pinkhams, and the Jaynes; for, after all, it is the minority that can be deluded by the flaring posters of "Wizard Oil," or the lying testimonials of "Tonic Vermifuge." When a sick man applies to a physician, thinking that thereby he will secure the benefit of special knowledge brought to bear upon the conditions of the individual case, entrusting to the conscience of his medical adviser his health and his life, he is entitled to the skill and the thought for which he pays, and that he deems himself to be receiving. He certainly deserves better treatment than to be handed over to the mercies of "antikamnia," or "febricide," or "quickine," or "gleditschine," or "Freligh's tablets," or "listerine," or any other of the unholy crew. If such is to be his fate, let him have the satisfaction of buying the worthless or poisonous stuff direct, without the sham of a professional consultation, and without paying a purchaser's commission to the medical sales-agent.

At the coming meeting of the State Society I purpose offering the following resolutions, for which I ask the support of this Society:

Resolved, That the Medical Society of the State of Pennsylvania hereby expresses its highest disapprobation of the practice of giving certificates or testimonials to secret preparations alleged to be of medicinal virtue, and calls the attention of the affiliated county societies to the fact that such action on the part of members of the said societies is in derogation of the dignity of the profession, and in violation of the letter and the spirit of the Code of Ethics of the American Medical Association and of this Society.

Resolved, That this Society likewise expresses its disapprobation of the practice of inserting advertisements of secret preparations in the columns of medical journals, such action being an insult to the intelligence of the profession, and a degradation of journals indulging therein to the level of the patent-medicine almanac. Especially to be condemned is the action of the *Journal of the American Medical Association* in admitting such advertisements.

Resolved, That copies of these resolutions, duly attested by the permanent Secretary, be sent to all county societies in affiliation with this Society, to the American Medical Association, to State medical societies in affiliation therewith, and to the publishers and editors of American medical journals.

On motion of Dr. J. Madison Taylor, the resolutions were adopted as the sense of the Philadelphia County Medical Society, and the delegates to the Medical Society of Pennsylvania were instructed to officially present and support them.

DOES ORGANIC DISEASE OF THE HEART PRECLUDE THE USE OF CHLOROFORM IN PARTURITION?[1]

By T. RIDGWAY BARKER, M.D.,

Demonstrator of Obstetrics in the Medico Chirurgical College, Philadelphia; Out door Obstetrician to the Pennsylvania Dispensary.

IN entering upon the discussion of a subject of such paramount importance to mother, offspring and obstetrician, one cannot lay too much stress at the very outset upon the axiom that "A good remedy will fail of its effect if not properly administered." This fact must be kept uppermost in our mind if we would avoid fatal results, not due, however, to the employment of the agent, as some would make it appear, but to the lack of attention and care exercised in its administration. That there is a radical difference between surgical and obstetrical anæsthesia (analgesia), goes without saying. If we consider for a moment the stages of anæsthesia, which differ only in the profoundness of the impression—first, sopor; second, stupor; and third, stertor—we cannot fail to notice that in analgesia one rarely has occasion to carry the effect beyond the first degree (sopor), while in the surgical variety we are obliged to advance beyond this and keep the patient in the second stage, or that of stupor, thus markedly increasing the gravity of the prognosis.

In this connection, let us devote a moment's consideration to the progressive effect of chloroform vapor upon the nerve-centers of the cerebro-spinal system, beginning, as it does, at the inferior extremity of the cord, sacro-lumbar, and gradually extending its paralizing influence upward until it reaches and expands its force upon the medulla oblongata. These well-established clinical observations having been verified by physiological experiment, we are justified in putting them to practical use. What other agent, may be pertinently asked, can relieve—aye, abolish—pain so quickly and safely, yet leave reflex muscular contractility unimpaired, as chloro-

[1]Read before the Philadelphia County Medical Society, April 27, 1892.

form? Ether and ethyl bromide have found favor with some practitioners, but neither can displace chloroform.

Fordyce Barker states in his writings: "I may say here that I have long regarded chloroform as the best and safest anæsthetic in obstetrics, and since 1850, I have used no other."

The danger from the employment of chloroform in this department of medicine depends more upon the carelessness with which it is administered than to any toxic effect inherent in it. The four cardinal points to be borne in mind when giving this anæsthetic are: First, plenty of pure atmospheric air; second, liberation of a small amount of the vapor at a time; third, attention to the respiration; and, fourth, frequent observations as to the force and frequency of the cardiac action. That the recorded cases of death have been due in a great measure to saturation of the residual air in the lungs to a fatal degree can scarcely be doubted. A few deep, forced inspiratory efforts will quickly produce such a condition. Withdrawal of the agent under these circumstances cannot prevent the further entrance of the chloroform vapor into the circulation, for it already fills the air-cells. Nor will attempts at artificial respiration prove effectual, since but a small quantity of the residual air can be forced out of the lungs, while that which enters fails to sufficiently dilute the vapor owing to the tardiness of diffusion. Let us not suppose, however, that because we administer to the parturient female small amounts of the drug continuously, therefore no risk is incurred, for experiments directed to solve this important question go to prove that even small doses, when continuously inhaled, tend to produce dangerous, and at times, fatal cardiac exhaustion. Far different is the result when given intermittently, as is the unalterable rule in obstetrics. Should we seek authority for the statement that the dangers from the careful administration of chloroform in labor are too insignificant to warrant its refusal, we have only to turn to the *American System of Obstetrics* to find therein the following: "The danger when chloroform is used only to the extent of mitigating or abolishing pain in childbirth is practically *nil*." Lusk, quoting from Bert's experiments, states "That chloroform might be intermittently administered for and indefinite period with safety." These remarks do not apply to its use in the third stage of labor, for, as is well known, after delivery of the child it is likely to occasion relaxation of the uterus, thus favoring post-partum hemorrhage.

Offering the above as a preface to my remarks on the judiciousness of employing chloroform when the parturient female suffers from organic cardiac disease, it now remains for us to consider the effect of parturition upon this enfeebled circulatory organ, thereby securing a scientific basis for our conclusions. In the first stage of labor we find the muscular contractions confined to the uterine muscular layers and directed toward overcoming the circular fibers of the cervix, while in the second or propulsive stage not only does the uterus exert its power to the utmost, but also the abdominal and respiratory muscles are brought into action by the will of the parturient in her efforts to expel the fœtus. The diaphragm is forced down and its movements paralyzed by the female holding her breath.

The other respiratory muscles are likewise unable to act, and hence imperfect oxidization of the blood results. As a consequence, the cardiac movements are accelerated, greater resistance is met with in the pulmonary and aortic circulations. Moreover, a tendency exists to venous congestion, as evinced by the hue of her face and swollen veins.

Owing to the excruciating pain experienced when the head passes through the cervix, the parturient is further tempted to make additional muscular efforts, which only augment the difficulties met with. Under normal conditions this strain is of such brevity that it cannot be considered of any importance, but when complicated by disease of the heart it is of far greater gravity. If the condition be one of fatty degeneration due to a previous peri or myocarditis, resulting in faulty nutrition and enfeeblement of the heart's action, as evinced by weak impulse, venous stasis, confused and irregular sounds, anæmia alike of brain and other organs, with faintness and oppression on the slightest exertion, this interference with circulation and respiration may readily tax its powers too far, and so cause speedy death from paralysis. Here the conditions which pertain in surgical anæsthesia are absent. The indications present are to allay excessive muscular action and respiratory spasm which is threatening the over-stimulated heart.

To allow the female to continue such efforts is to permit her to commit suicide; to warn her to desist is useless when in such agony; while delay is likely to be fatal. How can we overcome this condition of nervous excitement? Can we accomplish it by the administration of chloroform? Yes; of the two evils, for we must acknowledge there is an element of risk in giving chloroform, we can only choose the lesser, and so promptly proceed by inhalation to relieve the accessory muscles of parturition of their strain. By the abolishment of pain we lessen the work required of the laboring heart, which, instead of beating at the rate of one hundred and forty or more a minute, may diminish in frequency to ninety or one hundred.

What has been said of fatty heart is equally applicable to conditions of hypertrophy and dilatation.

The equilibrium, if disturbed, is almost certain to result disastrously. That sense of fullness in chest and oppression due to bronchial congestion, if relief is not afforded, becomes most distressing. The cyanosis from deficient aëration is greatly exaggerated, while the insufficient blood-supply to the brain causes syncope, and may be succeeded by coma if the excessive reflex disturbance be not removed. Nor are the indications for the administration of chloroform materially different in the case of females in labor with valvular disease. Whether it be mitral in the young adult or aortic in the aged primipara, the cardiac strain must be relieved if we would save our patient. As is well known, all forms of valvular disease ultimately develop a condition of ischæmia on one side with corresponding low tension, while on the other side is stasis with high tension. While by compensation life may run on for years, yet, when the strain of parturition comes, it will soon be overthrown if precautions are not taken to prevent it.

Of what benefit will be our knowledge of the value of cardiac "physiological rest," as laid down by Fothergill, if we do not apply it under these conditions? We all appreciate the importance of securing "quietude of mind and body" when such pathological states exist. Then why not employ the quickest and safest means to obtain it by the inhalation of chloroform? If the danger is great from "active exercise—climbing mountains, running up stairs, lifting heavy bodies, and all kinds of exercise involving heart strain"—how much greater, aye, how immeasurably so must it be when the parturient female forces, with the anguish of despair, every muscle to

its utmost in her desire to deliver her child. From a study of chloroform anæsthesia in obstetric practice we have seen how it should be administered and how it acts. Surely none will deny that in its employment under these circumstances we act otherwise than for the best interest and safety of our patient. That one may not be charged with being a blind adherent to theory, one has only to turn for support and justification to the teachings of the late lamented Fordyce Barker, who states: "It seems to be almost accepted as an axiom, with both the profession and public, that the inhalation of chloroform is dangerous for any woman with disease of the heart. For more than thirty years I have been convinced that this opinion is quite erroneous, and I have so taught in my lectures and in former writings."

He goes on further to say: "I have seen several cases, complicated by dangerous heart lesions, which terminated favorably, as I think, solely from the use of chloroform."

Snow, likewise, is of this opinion: "In all forms of valvular disease," he says, "chloroform, when carefully administered, causes less disturbance of the heart and circulation than does severe pain." To quote from Championnière: "If," he says, "I recognized an organic affection of the heart, without pulmonary complications, I should rather give the woman chloroform than to let her suffer." Were further proof necessary as to the propriety of employing chloroform anæsthesia, one might include among this group of clinical observers, Vergeley, who expresses himself thus: "Diseases of the heart are not a contra-indication to the use of anæsthesia." Macdonald states: "In almost all cases of heart disease with labor, chloroform has been given, and apparently with benefit, during delivery. If carefully administered I think it cannot but be useful in all cases." Since such eminent authorities advocate its employment, can we justify ourselves in refusing our patients the benefit and comfort this agent affords? What is the danger from chloroform compared to the state of exhaustion and collapse into which the parturient female will inevitably fall? If this heart is forced to the verge of paralysis from overwork and excitement, why shall we not use the means at our command to lessen that strain? Let us have a reason for the faith that is in us, and not hesitate to fearlessly employ extreme measures to overcome extreme dangers.

Chloroform by inhalation can and will, if properly administered, save the lives of parturient females, suffering from organic disease, when death seems imminent from over-stimulation of its ganglia through reflex nervous action. Organic heart disease, then, does not preclude the use of chloroform in labor, but rather is a condition calling for its careful administration.

A GERMAN physician, after carefully analyzing six hundred and thirty cases of syphilis treated in the medical clinic of the hospital to which he is attached, has found that thirty-four arose from extra genital infection; twenty-three of these were on the lips; one on the tongue; two in the mouth, and three on the mammæ. Three were on the genital organs, without evidence of sexual congress; and the remaining two could not be located. These investigations show that 5 per cent. of all cases of syphilis are due to extra-genital infection. Many authorities give the maximum as 10 per cent. In certain parts of Russia the proportion is said to reach 80 or 90 per cent.

A CONSIDERATION OF ACTINOMYCOSIS, AS TO ITS NATURE AND RELATION TO THE PUBLIC HEALTH.

BY FRANK S. BILLINGS,

Director of the Patho-Biological Laboratory of the State University of Nebraska.

[CONTINUED FROM LAST WEEK.]

It is not speaking too emphatically when I assert that every action, or want of action, of the Illinois Live Stock Commission absolutely and unequivocally gives evidence that they do not really believe one single word of their assertion that lump jaw is a contagious disease, as they manifest it in every act they do when combating diseases which really have that character.

Furthermore, when on the stand, and under oath, at Peoria, every one of their veterinarians, so called experts, testified it was his belief as well as generally admitted by all who had any experience in the matter, that the primary origin of actinomycosis, the original habitat of the fungus, was in some form of grain or grasses at present not positively known. That one admission knocks their assertion that "lump jaw is a dangerous contagious disease" into smithers.

Did any mortal human, since history's beginning, ever hear of; has medical investigation in any land ever discovered, or is there any record of a contagious disease having its source of primary origin in a parasite affecting vegetation of any kind?

I challenge the record on that point, Let us now turn to another side of this question for still more positive evidence that actinomycosis is not a contagious disease.

FACULTATIVE PARASITIC DISEASES.

The true and scientific, as well as practical hygienic reasons, having been given for designating certain diseases as contagious, or obligatory—parasitic, because their germ cause only finds the conditions of nutrition suitable to their own lives, in the chemical constituents of animal organisms—we now come to that other great class, the "facultative parasitic," or the infectious diseases of ancient writers. The term facultative parasitic should carry its own meaning to every intelligent reader; it is that the germs causing this class of diseases are not of necessity bound on animal organisms, or their tissues or fluids, for their biological necessities (food). They are not obliged to be parasitic—that is, living on or at the expense of other organisms. They can live there, that is all! They have the faculty of adapting themselves to the nutrient conditions afforded by living organisms. Their natural habitat, however, is the earth, cesspools, earthy material—that is, in conditions offered by the surroundings of animal life. The diseases caused by such germs are known as exogenous, generating from without the animal organism, or, as I have termed them, extra (outside) organismal in character, which is the same thing. Here, then, we have the only essential difference between a contagious disease (endogenous) and an "infectious" disease (exogenous), in the sense of old writers and in the sense of hygienic or preventive medicine. The modern words correctly and logically express the true meaning, while the older ones do not, unless we know what we are writing or talking about.

Otherwise the two classes are very much alike in many points.

Nutrition plays as essential a rôle with regard to the germs of the facultative parasitic or exogenous diseases as with the obligatory parasitic or endogenous, but with far more freedom of selection, or adaptation to surroundings. These facultative bacteria, first of all, have not that delicacy of constitution which requires that the temperature in which they live shall be nearly constant at one point, or vary but a very few degrees from the same—that is, the temperature of the animal body, which is from about 99° to 102° Fahrenheit. On the contrary, they will develop in any temperature from a few degrees above freezing to the highest temperature of the animal body. Again, they are by no means so select in their choice of food; they develop in the natural earth, in filth, on and in dead bodies, as well as in the various artificial media, with considerable ease and certainty. In some things, however, they are very delicate in their selection of food or the conditions under which they develop, but this is equally true of the obligatory germs. Most of them do not thrive in acid media food, nor do they in one having a very strong alkaline character. Some require oxygen to live, others do not, while still others will live with or without it; some will not grow at all in oxygen, while others require hydrogen gas to develop, and still others require carbonic acid gas, which is poisonous to animal life. It is thus to be seen that germs, like higher organisms, may be quite select in electing the conditions under which they live. It is my opinion that the germs of the eruptive fevers (scarlet, measles, etc.) in man require carbonic acid to develop in, as they do not thrive well in the blood, but produce their chief lesions in the cutis, where the circulation is the poorest and the accumulation of carbonic acid the greatest. I think it is because investigators, having the opportunity, have not persistently tried to develop these germs in this gas, that they have failed to cultivate them. As we ourselves only take such food as we want, and the digestive organs of our bodies can only handle that which is useful to us, so, like the Pharisees in the Bible, these germs "pass by" on the other side of those things which they do not need as food; they select what they want and let the rest alone. The trouble with us investigators is, that we cannot always select what the germs want, or so adapt the food we offer them to their necessities, that they can select the useful and leave the balance alone. We shall improve as we continue to learn. We can change the character of germs by prolonged exposure to certain kinds of food. We can increase their so-called virulence—disease producing power—or take it away altogether by changes in the conditions which we offer them to live in. Sometimes it is another degree of temperature, and again changes in the chem ical constituency of the food we offer them. In one case, at least, a natural accident has changed the character of a germ so as to make it invaluable to the preservation of human life. I allude to the germ of small-pox. When considered as the germ of that disease, we all dread it for its extreme virulence, giving to the disease its specific contagious character; but when we think of vaccina—cow-pox—it is not with a feeling of dread, but of grateful thankfulness to the genius of the immortal Jenner. We have no dread of vaccinated children. The germ of the disease in man and cattle is the same, but through the accidental transmission (perhaps for centuries before Jenner made his observations, when small-pox itself was one of the terrors of the world) of the disease from cow to cow (milk cows generally have slight abrasions of the skin of their teats) for generations by the milkers, a different medium, chemically (food), was constantly given to the germ, in which it thrived, but so changed its character as to lose its entire virulence, it did not produce a contagious disease in cattle or man, and does not to day, *though it can be transmitted* to either, and indefinitely further in healthy individuals of either species. This fact is especially called to the attention of those imbecilic, illogical or ignorant medical men or investigators who assert *transmission to be identical with contagiousness*—I speak in the genetic sense only. Something, on the other hand, was retained in this bovination of small-pox. That invaluable something is the preventive power (without virulence) of vaccina. Nature has done, through the accidental and unreflecting assistance of man in this one disease, that which is at present beyond the power of man to do with the known germ of any other non-recurrent disease of man or beast. As said above, we can do what we please in regulating the virulence of known germs by changes in the conditions we offer them to live in; but while the germs of non-recurrent diseases must necessarily also have a preventive power (or second attacks would be the rule instead of the exception), we know of no means of so cultivating such germs artificially as to preserve this chemical protective product without the virulence. Could I only do that, the problem of an absolute and constantly unvarying preventive virus against swine plague would be solved. I do not despair. It is objected, by the Bureau of Animal Industry, that I publish almost entirely for the farmers and in public prints. That's queer! For whom do they issue their hitherto useless reports? This work is paid for by, and being done for, the people, not for the self-congratulatory amusement of a few investigators. It is certainly not being done for the edification or education of the government dilitants, though they are slowly beginning to learn its value and to profit by it. If Nebraska continues to support it, they may eventually learn something in Washington. "The vilest sinner may return," so it's said. Machine politicians are not mentioned, however, as having a chance for a change.

It will be remembered that we are discussing those germs which are not necessarily the cause of infectious diseases, but which have the faculty of becoming such. These are exogenous germs, that is, such as primarily live and find their natural home outside of any animal organism. In reality, they are far more important than the obligatory parasites, or the germs which thrive only in the animal body.

The study of their lives is also far more interesting. While the obligatory parasitic diseases have been lessened in number by our hygienic endeavors and are being brought under more direct control, facultative parasitic are in danger of being increased. The cause of this is to be sought in nutrition. It is one of the penalties of civilization, of ignorance and carelessness, of the aggregation of animal life in one place, and grouping densely; of the saturation of the soil with animal excretions, urine and fæces. This group may be called the penalties of civilization. Another group may be termed, the natural facultative parasitic diseases, because they are not directly produced by the influence of any act of man, though they may be transported from place to place by him as any other seed can be. They are spread like our weeds. Here again, civilization pays its penalty. Let us tabulate a few of these two groups and then consider them apart:

NATURAL FACULTATIVE PARASITIC.	ARTIFICIAL FACULTATIVE PARASITIC DISEASES.
Yellow fever.	Typhoid fever.
Texas fever.	Diphtheria.
Fever and ague.	Rouget of swine (in Europe).
Swamp malarial fevers.	The horse distempers.
Anthrax.	Swill and filth diseases in swine.
Black leg.	Cerebro-spinal meningitis.
Mad itch.	An unnamed kennel disease in dogs.
"Wild-seuche" in swine, cattle and deer.	Hen cholera.
	Swine plague.

What then is this difference?

It is plainly evident if we give our attention to both these groups of facultative parasitic diseases, as they occur under natural conditions, "in the field," as we say when speaking of the study of animal diseases. It cannot be discovered by any laboratory investigations, however, the misfortune of which has been to so magnify their importance that the real study of diseases under the conditions in which they daily occur has been pushed into the background, and too frequently neglected altogether.

In facultative parasitic diseases of the "natural" type it is evident (illustrated by yellow fever, Texas fever, anthrax, etc.), that the saturation of the soil with animal excreta is not necessary in order that the bacteria causing them shall be virulent to man or beast. These germs are naturally so. They find the conditions to such virulence in the nutrition offered by the virgin soil itself under certain local chemical and climatic conditions. They find equally favorable nutrition in the organisms of man or susceptible species of animal life.

The other group is entirely different. They inhabit certain forms of virgin soil and find their natural home also under certain local and climatic conditions. But, under such circumstances, they are as harmless as a cooing dove. Man may take them into his system (the same of animals), and they pass out again doing no harm. But when man has settled on such soil, and it has become saturated with the chemical products of human as well as animal excretions and filth, these germs change their character; they become virulent, in other words, they produce a substance themselves which experience shows is toxic or disease, producing in certain forms of animal life and not in others. Why is this? Attention has already been called to the fact that like every other living organism, germs are also selective in reference to their food; they pass by that which they do not want. Regarding this virulent, toxic, or disease producing property it has also been noticed that we ourselves can influence it very easily in any desired direction by changes of food in the laboratory. It is not too much to assume that the same thing occurs in nature though it may have taken centuries to bring it about, and has not come about until after years of saturation of the soil with the chemical elements of the animal body passed off in the excretions. A careful study of the diseases afflicting man in his most natural, simple and isolated condition will, I think, most successfully demonstrate the correctness of these conclusions. In this class of diseases, as well as the natural facultative, and also endogenous diseases, we also find this elective, nutritive attribute displayed, in that certain germs gain entrance into and grow and produce disease in certain species of animals, while others that are fully as much exposed to their action are not in any way injured. In swine-plague in a herd of two or three hundred hogs, the animals eating the same fodder polluted with their excrement, given an equal number of cattle, it is absolutely impossible that the cattle should not also take the germs into their intestines, so of horses in the same yard. And I have seen hens and ducks eating their fill of dead hogs many a time, and yet not one of these other animals ever sickened in hundreds, yes thousands of such experiences in this western country.

The most striking example that has come within my personal observation of the influence of changes in nutrition under natural conditions, is given by the practical result of the disease of corn to which I have given the name of "Corn stalk Disease," and in which I successfully demonstrated the germ during my previous term of service in Nebraska, though, as in every other discovery made by another investigator, that notorious fraud, the Agricultural Department at Washington, has deemed it an (im) moral obligation to cast doubts upon it or pronounce it erroneous altogether. So far as practical experience and observation extends the germ of this disease of corn (it has been described by Burrill, of Champaign, Ill.), though manifesting itself in the green and growing corn, is absolutely harmless to cattle, or other susceptible stock, so long as the corn is green. It is only when the corn is dead, towards the latter part of October or in November, that the germ in the corn becomes dangerous to cattle. Basing my hypothesis upon this annually occurring practical observation, I assumed that during the slow process of the death of the leaves (the germ in virulent form has as yet only been demonstrated to exist in the leaves and husk of the cob), the chlorophyl undergoes certain chemical changes, thus altering the nutrient conditions offered to the germs and supplying them food out of which they secrete some toxic or disease producing stuff, which property is also retained by them, when they gain entrance to the bovine organism.

It seemed very essential to endeavor to prove this to be so by actual experimentation. Thanks to the publication of Prof. Burrill, we were enabled to recognize the disease in the growing corn, a plot of the same having been discovered in the past summer by Mr. Duncanson, assistant to Prof. Ingersoll, Agriculturist of the University of Nebraska, and with the co operation of Prof. Bessey, of the same institution, I was enabled to get what proved to be the desired material, a full description of which, with most exact illustrations, will eventually appear from the hands of that accomplished botanist.

The first corn brought to me was green, though somewhat discolored in spots, but it is not my province to describe the lesions presented. From the lesions I isolated a germ which, so far as could be judged, was the same described by Burrill, as well as that previously obtained by me in the organs of cattle that had died after being turned into stalk fields. The inoculation of rabbits with pure cultures of this germ at this time (August, 1891), as well as feeding others with the diseased leaves, produced no effect whatever. Towards the last of September another batch of stalks from the same field was brought to me, more discolored and wilted than the previous ones, but still sappy and greenish. The same germ was again isolated and similar inoculations and feeding experiments made, with the result that the rabbits were slightly ill and off their feed for a day or so, but nothing serious. On December 10, Mr. Duncanson again brought me stalks from the same field, this time both stalk and leaves being dry and dead. No attempt was made to isolate the germ from the dry leaves, but they were stripped and fed to rabbits, plenty of water being given them. Two rabbits died on the 12th

of December, their blood being replete with the previously mentioned germ, as shown by comparative cultures in and on different media, though I consider the source of the germ and the well-known conditions under which it was obtained, and the experimental results, the only reliable data on which to establish identity in such cases.

These results, I think, are not only very instructive but absolutely conclusive of the correctness of my previous hypothesis as to the changes in the chlorophyl causing the change in character of the germ, through its changed food, thus making disease producing in cattle.

To the feeder they say in no uncertain language raise fodder for your stock, cut it green, and do not turn it into stalk fields until you can learn how to distinguish the desired corn and cut it out.

This is another strong point for the silo!

This corn fodder disease is transmissible from animal to animal by inoculation. I should like to see the fanatic who would dare affirm that it should therefore be classed as a contagious disease.

It has almost been impossible to satisfactorily produce typhoid fever in any of our domestic animals, and, as a natural disease, does not occur in them. This immunity of one species to the germ causing most malignant disease in another, can be daily demonstrated by actual experimentation. Even a more singular thing is that germs can be isolated and cultivated from the intestines of animals which, while there, do them no harm, but which if injected into the blood of an animal of the same species it will kill. Then there are a large number of germs in the mouth or intestines of animals that are experimentally fatal if inoculated into one or the other species of small animals used in laboratories, and yet are harmless where originally found.

These artificial facultative parasites only cause specific disease (become parasitic) in that species of animal life, the chemistry of which organism is the same as that of the excreted chemicals in the soil that have changed these same germs from non-virulent harmless organisms to most virulent facultative parasites.

This saturation of the soil with animal (human) excretions is the real cause of typhoid fever, diphtheria, swine-plague and the diseases of this group; hence, they may rightly be termed the penalties of civilization. It is the same conditions which cause the fatal stable diseases in horses in our city stables.

This is why cities like Chicago are fast becoming unfit to live in, and why we are learning to give so much attention to sewerage and drainage and the care of all excreted materials. Plenty of sun, and the most perfect and deep seated drainage possible with the most complete removal of all the excreted materials of animal life, must be the aim of our hygienic endeavors. Swine-plague was never imported into this country. Its germ is a natural inhabitant of all our moist, heavy soils, but not of our sandy, gravelly, or shallow soils. When we began to raise hogs in great numbers, and for a series of years in these sections of the country, the swine-plague first appeared, and has been growing worse ever since, in such sections. In sections having a dry, sandy, or gravelly soil it does not primarily originate, the germ is not there, or if there, the nature of the soil is such that it does not hold the chemical elements in sufficient degree of saturation to change the character of the germ. Even if the malignant germ is carried into such sections by infected hogs, the disease does not become permanently planted there.

To sum up:

A "contagious" obligatory parasitic or endogenous disease is one that finds its only source of primary origin in a diseased individual of a given species of animal life, while an "infectious" disease (in the ancient sense and hygienic meaning of to-day) is a facultative parasitic, or exogenous disease, or one which as invariably finds its primary origin in conditions outside the animal organism.

From this genetic, primarily hygienic point of view, every other characteristic of these diseases is of no importance whatever.

THE WORD CONTAGIOUS A HUMBUG.

The resemblances, or similitudes between endogenous and exogenous diseases.

We come now to the discussion of the word contagious in an even more exact manner—in order to show that unless used in the patho-genetic, or primary disease producing sense, already so fully explained, that the word is a miserable one, and in any other sense has no scientific or special meaning whatever. So misused has it been and still is in its application, that the best thing would be to damn and ban the word "contagious" from medical literature altogether. Taking the roots of the word itself from "con"—with; "tangere"—to touch, or to come in touch or contact with a given something, it is easily to be seen that the word has no particular or limited application whatever, and this is the reason that its meaning has become so indefinite in modern medicine that a person must have a clear and positive idea of the whole subject another is talking or writing about, before he can form any definite conclusion as to what such a person means when he uses this abomination to every educated and logical student of the phenomena of disease. Let us see how absurdly, yet how justly, according to its root derivation this word "contagious" can be used. You go into a blacksmith shop and carelessly pick up a horse shoe from the floor that has cooled off enough to look black, and it burns your hands severely and an intense inflammation follows. It is absolutely correct for you to say that that piece of iron was a "contagion" and the resulting inflammation a local contagious disease, for did you not come in touch with it, and did it not produce disease? You go out of a night and drink too much whiskey, and derive an acute inflammation of the stomach for your dissipation. The whiskey is a contagion and the inflammation contagious, for your stomach comes in contact with too much alcohol. In both cases you acquire an inflammation through coming in contact with a given something. Now, an ordinary layman would never for a moment think of speaking of any disease acquired in that way as contagious, yet it is with just such absurdity that the word is used by otherwise well educated and unquestionably competent physicians, as illustrated by quotations from men of unequivocal reputations.

Burdon Sandereon is one of the first medical writers in England. Recently, he delivered a series of lectures before the students of one of the London medical colleges upon inflammation which have been published in late numbers of the *Lancet*, and from which I quote the following:

"Up to the year 1860, it was assumed that whenever inflammation occurs in consequence of an injury, the apparent cause of the morbid process is the real one. Since that time it has gradually become clear that in the majority of instances this is not so; and that, however direct and simple may be the relation which follows, the result does not occur unless there

be present at the seat of the injury another condition —something analagous to the contagia which are the acknowledged indispensable agents in the production of specific diseases. So that, in all these cases we have to distinguish between the contagium or proximate cause and the mechanical or chemical injury by the co-operation of which it is enabled to act. Now, the pathologist who first referred to contagium as an element in the causation of inflammation was John Simon. He adduced many striking facts derived from chemical experience as reasons for believing that "generic and essential property of inflammation that its actions (or some of them) are always, in their kind, to some extent contagious. The most important of these facts were the readiness with which conjunctivitis can be produced by the introduction into the conjunctival sac of any kind of inflammatory production of disseminated inflammations by intravenous injection of perfectly healthy pus, and in general all those cases in which traumatic inflammations—such, for example, as that produced by running a splinter of clean wood into the finger—give occasion to inflammation of the corresponding lymphatic glands. All such instances seemed to Simon to indicate that even non-specific, clean, uncontaminated inflammations are more or less spreading, or contagious."

Greater nonsense than the above was never printed, even in the columns of a daily paper. According to the idea there expressed a disease is "contagious" if it can be artificially transmitted from individual to individual, or a local disturbance is contagious if it extends from its primary seat to neighboring tissues or organs. The fact of the matter is, that in the only logical sense in which we are warranted in using the word that neither transmission from individual to individual, whether per contact or not, or by intentional inoculation or not, or whether a disease extends over the body from a primary local center or not, has no more to do with the actual meaning of the word "contagious" in the true and only sense in which it should be used than with the man in the moon.

It will be remembered that it is on the ground of the possible transmissibility of actinomycosis from an animal afflicted with lump jaw by the act of inoculation in the hands of an experimenter, that the Illinois Live Stock Commission have pronounced lump jaw to be a dangerous contagious disease, or to use their own words:

"It was clearty demonstrated that the fungus—actinomycoses—could be transmitted from one animal to another by inoculation, which fact establishes it as a contagious disease."

This fact was never scientifically and surely demonstrated until 1891.

Another veterinarian who has posed as a great authority in this country and supported the above assertion of the Commissioners of Illinois, but whose great authority is more due to the position he occupies at Cornell than his actual knowledge of pathology, is James Law, who made the following similar absurd statement in connection with the contagiousness of actinomycosis.

"I well remember that twenty-four years ago Dr. Burdon-Sanderson, physician to the Brompton Hospital for consumption, London, argued as does Mr. Mollison, that attendants had been in that institution for forty years without contracting tuberculosis, and, therefore, he could not believe the disease to be contagious. He went to work to disprove the offensive theory by the crucial test of inoculation, and after sacrificing scores of rabbits and guinea-pigs, and extending his investigations over several years, he was compelled to acknowledge that the error had been with himself, and that tuberculosis is essentially contagious."

Let us now apply the "crucial test" of good everyday common sense to both of these assertions. Suppose that rabbits and guinea-pigs had been used to demonstrate the crucial test in both actinomycosis and tuberculosis, and the inoculation had failed completely, and so far as that goes were still an impossibility, would the failure of inoculation, artificial transmission, be one iota of proof as regards the natural character and origin of either disease? To argue that it was not until this modern method of the artificial inoculation of small animals (in which a given disease does not naturally occur) was adopted in experimental medicine that the demonstration that a certain disease was first proven to be contagious, is to insult the common sense and observational powers of some of the greatest physicians and hygienists that have ever lived.

These modern methods of experimental transmission in small animals have come in vogue because investigation has gradually led to the discovery of the actual causal moment in many diseases, and found them valuable (to a degree) in studying the development of such moments—germs—and not that such transmissions had a particle of necessary relation to the primary origin of a disease, or its actual course in that species (or those) in which under natural conditions the germ is the cause of a specific disease.

Long before even the great John Hunter performed inoculations between persons, and from a primary lesion (chancre) to a healthy portion of the same person in syphilis, it was evident as then that the disease was contagious, and found its origin in man only, and passed in some way from man to man. The combined failure, by all investigators to transmit syphilis to other animals save man and produce syphilis, in no way influences the well-known fact that syphilis is an endogenous (contagious) disease. Do we need any laboratory, or experimental proof, in any other animal, or even any experimental transmission in the horse, to strengthen the fact that glanders primarily originates in the solipeds—horses and asses? It is self evident that we do not. Does the fact that glanders is not transmissible to cattle alter the natural condition at all, so far as the origin of the disease in solipeds only is concerned, and is its contagiousness any more strengthened in that species because it can be transmitted to guinea-pigs, rabbits, man, dogs, and other animals? Is pleuro-pneumonia of cattle any the less a contagious, endogenous, disease in the bovine species because it has never been transmitted to any other form of animal life, either accidentally or experimentally so far as I know, though, if I remember correctly, some form of pleuro-pneumonia has been said to occur in goats, but there is no proof that was the specific contagious disease of cattle?

Even in the inoculation of cattle against lung plague the specific lesions in the lungs are not produced. According to such authorities (?) as have been quoted there is no proof of that disease being contagious in cattle, for the "crucial test" is wanting. Because some inflammatory product can be carried into the "conjunctival sac" of the eye socket, as Burdon-Sanderson asserts, is not a particle of proof that there is anything "contagious" in a simple inflammation; even though we can isolate one or the other of the pus-cocci from the inflammatory products produced and inject the culture under the skin of a susceptible small animal and kill it, still that act of transmission may be and generally is absolutely

without value in throwing any light on the primary origin of the coccus, or the nature of the inflammation in the original subject. Away with such nonsense! Let us be of some credit to a profession claiming a place among the "learned professions," and not continually and persistently demonstrate to a better informed lay public, at best one having some common sense, that so far as the logic of pathogenic sequence is concerned that we are little better than a profession of confirmed idiots.

Every practical act of any hygienic body of competent physicians completely contradicts this laboratory-transmission nonsense. We look first at the source from which the danger originally came, and then at the chances for transmission from the diseased individual to healthy ones, but the "transmissibility" bears no necessary relation to the primary origin, and no disease on earth can be classed on such a foundation, nor differentiated from another.

There is not an observer or authority who must not and will not admit, that all contagious diseases are infectious, but there is equally well not one who would dare affirm that all infectious diseases are contagious.

What constitutes the difference, or on what characteristics of these diseases does this universally admitted differentiation bear, and still is, based? Had the fact of transmissibility either in the species in which a natural susceptibility exists, or the experimentally demonstrated possibility of transmissisn to rabbits, guinea pigs, or what not, an iota of influence in determining this universally recognized differentiation of infectious diseases into two classes? Emphatically no! Again, I ask, what has caused and still causes such distinguishing between them?

Every one who has a healthy, educated brain, and is not suffering from a laboratory psychosis, knows that this division is based on the primary origin—original source of danger—of the two classes and not upon secondary possibilities. Not to the diseased individual present, but to the source from which, in the first place, that individual became infected, and which has been, so far as is known, the only source from which, or in which, such a disease finds its primary genesis.

The folly of the use of the word "contagious" is illustrated by many endogenous and exogenous diseases having numerous common characteristics.

"Contagious" logically means to come in touch with some infecting medium, nothing more! Contagiousness and transmissibility are one and the same thing. It has no necessary bearing on the origin of the agent transmitted, however, and as the word contagious has had an old and well established meaning which ignorance has partially robbed it of, it should be dropped altogether, as in its present use it is only misleading and false. This is best demonstrated by considering it as identical with transmissibility.

An example: A horse is turned out in a certain pasture in which is some low, moist land; the grass is dried and hard except in this moist place, the animal seeks out this spot and grazes there, and in two or three days dies of anthrax; a veterinarian is called in to make an autopsy; his hands have some scratches on them and he acquires malignant pustule, as anthrax is sometimes called in man. What has happened? The veterinarian has been in contact ("contagious") with a diseased horse; having had wounded hands this contact has led to the transmission of bacillus anthracis (the germ) from the dead horse to the man. In the modern sense then, anthrax is a contagious disease because it is transmissible.

Our veterinarian recovers and again is called in to examine a horse suspected of glanders. Again, through some accident, he has wounded hands, and in feeling around the nostrils of the horse gets some of the disease secretion in the wound. By this accident of contact and an opening in the cutis—he has come in contact with the bacillus of glanders, which has thus been transmitted to him. He dies this time, though that matters not in the question. Here again we have contact—contagious, and transmission.

Who will dare tell me that anthrax and glanders are diseases of the same character because of this identity in the characteristics of transmission and infection? How then, where then, shall we look for a logical point of differentiation?

To the seat of primary origin of the respective germs in each case. The horse that had anthrax found the bacillus in the soil, hence it is an exogentic disease, bacillus, but has the faculty, however, of living in some form of animal life. The fact that from that horse anthrax could be transmitted to healthy horses or rabbits, and cause the disease in each one is of little or no value, however. We can stop the danger by burning the first horse up, and keeping others out of that portion of the pasture, or fencing it tightly up, or in time by good drainage. The glandered horse, however, acquired the disease from another horse, that is all. We must trace the transmission back from horse to horse until we find the first one, and that being the last glandered horse in the country, we can make an end of the disease, if no other gets in. Who cannot see the vast difference, and that the word "contagious" in the modern usage, is an insult to common sense?

This is why all contagious diseases are infectious though all infectious diseases are not contagious.

WHAT IS AN INFECTIOUS DISEASE?

As it is now admitted, even by the Illinois Live Stock Commission, that actinomycosis finds its primary origin in vegetable life, and as it has been demonstrated that any disease having that character or origin, cannot possibly be "contagious" in the only sense in which that word can be logically used in medicine, it is evident on that basis that the Illinois Commission has not an iota of authority to condemn a single steer, no matter how many lumps he may have on his jaws.

The question now before us is, actinomycosis not being a contagious disease, is there any evidence that it is an infectious one?

It will be remembered that it is a generally admitted fact that, while all contagious diseases are infectious, all infectious diseases are not by any means contagious.

This very admittance, which is absolutely recognized by every physician of any education whatever; that is, that there is a difference, completely knocks the ground from under the assertion that actinomycosis is a contagious disease. Remember that the only ground for such an assertion, upon which the Illinois Commission and its authorities make their stand is, that it has been demonstrated that the actino fungus is transmissible by inoculation from one animal to another. Now it is submitted, evidence enough has been and will be also submitted, that transmission, either natural, accidental or experimental, gives no logical ground upon which to differentiate infectious diseases, and that it is not now and has never been the reason which has influenced medical observers from the earliest dates to recognize the fact, that

"while all contagious diseases are infectious all infectious diseases are not contagious."

The failure of experimental transmission proves nothing in the case, nor does the natural fact that a contagious disease is transmitted by natural or accidental means. Both classes can be transmitted, sure, in that species of animal life in which they occur as a natural infection. Either class can be transmitted to other animals, in which the disease does not occur as a natural infection, if the nutrition offered is favorable to the life of the germ and the development of its toxic qualities. Neither class can be transmitted when such nutritive conditions are unfavorable. Surely, then, it is evident that there must be some other and only one other reason for this differentiation.

What is that reason?

Source of primary origin alone! Whether the given disease is endogenetic or exogenetic? As by the admission of the Commission's authorities, actino never primarily develops in the bovine organism and according to all medical investigators in the human organism, but on vegetation they at once demonstrate the difference between it and other infectious diseases and that it cannot be contagious.

Further argument or evidence should be unnecessary, but I intend to grind this nonsense down so fine as to leave no edge or point for these mistaken people to rest upon.

This brings us more closely to the other side of the question, the hygienic, or the relation of the meat from lump-jaw cattle to the health of the consumer. We will, however, only prepare the way for the discussion of that question at present.

We have now to consider what is the generally admitted and specific characteristic of an infectious disease?

It is this, that some living organism—a germ—gains entrance to an animal organism and lives and develops therein, during which time it produces a specific something (poison) which, being taken up by the blood, causes a general pollution of the organism, that is a general disease which is manifested by certain lesions common to all infectious diseases.

The phenomena of this class are first, fever, or a rise in temperature; second, certain changes in the great glandular organs—liver, and kidneys, and muscles, and musculature of the heart. These are the common lesions in which one infectious disease varies only in degree from another. On the other hand each infectious disease has something specific to it, some organ or tissue shows itself to be peculiarly suited to the action of the germs; thus the eruption in scarlet fever and measles, the specific lesions of syphilis, the diseases of lungs in contagious pleuro-pneumonia, the ulcerations and other products in glanders, the lesions on the feet and in the mouth in the contagious foot and mouth disease. In actino we certainly have the local disturbances in the jaw, the tissues around it, and other parts of the body. The question is, Do we have the general disturbances common to all infectious diseases? Do we necessarily have fever in actinomycosis? The second is, can actinomycosis be permanently cured by a surgical operation, for if so there can be no general disease? In other words, if the lump is removed, does the disease go on and display itself by other lesions over the body?

At the State farm we have a cow that has had actinomycosis for over five years, and been among the other cattle all the time, and yet there is no other case among them. We will only consider her as to that universally common phenomenon in all infectious diseases—*fever* only.

For this purpose her temperature has been taken, with that of two others in the herd, every day for two weeks. It should be mentioned that a more extensive case of lump jaw has seldom been seen by any one.

	Shorthorn Cow Calved Jan. 18, 1885.				Shorthorn Heifer Calved Jan. 10, 1889.				Lump-Jaw Cow Born Jan. 7, 1881.			
Dec. 1.....	A.M.	38.80	P.M.	39.20	A.M.	39.50	P.M.	39.30	A.M.	39.30	P.M.	39.10
" 2.....	"	——	"	39.10	"	——	"	39.10	"	——	"	39.10
" 3.....	"	38.40	"	38.40	"	39.50	"	39.40	"	39.10	"	39.30
" 4.....	"	39.10	"	38.65	"	39 80	"	39.30	"	39.20	"	39.00
" 5.....	"	39.30	"	38.80	"	39.70	"	39.80	"	39.20	"	39.10
" 6.....	"	38.70	"	39.00	"	39.70	"	39.60	"	39.20	"	39.15
" 7.....	"	38.60	"	38.70	"	39.90	"	39 50	"	38.90	"	38.70
" 8.....	"	38.90	"	38.80	"	39.90	"	39.50	"	39.10	"	39 20
" 9.....	"	38.70	"	39.00	"	39.50	"	40.50	"	39.00	"	39.20
" 10.....	"	38 90	"	38.90	"	39.70	"	40.00	"	39.20	"	39.80
" 11.....	"	38.90	"	39.00	"	39.60	"	39.80	"	39.05	"	39.10
" 12.....	"	38.90	"	40.80	"	39.90	"	40.10	"	38.90	"	39.30
" 13.....	"	39.10	"	39.00	"	39.90	"	39.95	"	39.00	"	39.10
" 14.....	"	39.00	"	——	"	40.00	"	——	"	39.20	"	——

The above given measurements were taken, as can be seen, with a centigrade thermometer. The normal mean temperature for cattle being from 38 to 39.50 or from 101 to 102½° Fahrenheit. It is at once to be seen that the temperature of the lump-jawed cow was not any higher than that of the two looked upon as absolutely healthy, and not above the normal limits, while that of the heifer, as is usually the case, was the highest of the three. Here, then, we have conclusive evidence that nothing existed in the lump-jawed cow of a general infectious nature; that actinomycosis does not, itself, produce any specific chemical irritant, and, hence, cannot be a specific infectious disease.

Again, it is now generally admitted that the actinomycoses, when unpolluted, do not produce pus, so this bars out another condition of infection. When pus is present, the pus germs are mixed with the actinomyces. It may now be said that actinomycosis is not an acute infectious malady, but rather a chronic infectious (contagious) disease. Let us, then, consider it from that point of view. Syphilis, while often an acute contagious disease, is also a very chronic one. In either case, however, it is an infectious disease, as are all contagious—endogenous—diseases. Some years ago it was asserted, and most desperately hoped to be true, that if the primary lesion, chancre, was surgically cut out when it first appeared, that generalization—infection—would thereby be prevented. This idea has been completely exploded, and the fond hope blown to the winds. The general infection invariably follows, though complete healing may rapidly result where the primary chancre was cut out and nothing again appear at that point; but the general infection as invariably follows, as said before. How is it with actinomycosis in this regard? Unfortunately, we have no exact and reliable data regarding the operation in cattle, but it is perhaps all the more fortunate that human medicine is replete in reports of so many cases of lump-jaw successfully operated upon that it can be stated as a law, that wherever the actinomycotic lesion is so situated that it can be readily got at with a knife and completely removed, that a permanent cure will result. On this point I have not yet had time to collect all the cases reported in the literature of our library, but a sufficient number have come to hand to answer every practical purpose, and that is all we need at present.

In the London *Lancet*, 1888, Vol. II, p. 176, two cases of lump-jaw, one in a man and one in a woman, are given as successfully operated upon by Barez in

Germany. In the same journal, Vol. II, 1891, pp. 1, 115, "three highly interesting cases" are reported, and the editor says: "The chief lessons derived from them are:

"1. The readiness with which the fungus may be detected in the discharges from the lesions.

"2. The tendency of the growth is to localize and not disseminate.

"3. The suggestion, almost amounting to a certainty, of the cause of the disease originating in cereals—vegetation."

All these recovered after operation.

Bostroem gives the following cured cases:

1. Actinomycosis of left upper jaw.
2. Actinomycosis of lower jaw (mention that the patient had no fever).
3. Actinomycosis of lower jaw.
4. Actinomycosis of lower jaw.
5. Actinomycosis of pharynx and adjoining tissues.
6. Actinomycosis of lower jaw.
7. Actinomycosis of lower jaw.

Senn, of Milwaukee, gives a case of actinomycosis of upper jaw in which complete recovery followed.

W. Fischer's case of actino of the tongue, noticed earlier in these papers, was completely cured.

Baracz gives 9 cases, mostly of the jaw, which were permanently cured where a radical operation could be performed.

Lunow says that of 150 recorded cases, 75 have been cured, and gives 4 more cured cases, and Mattakowski another. Israel reports 3, Roser 2, and Kepeller 15 more, all permanently cured. Bertha, a case of intestinal actino, cured. Helferich, 2 more in same locality, also cured; Leser, 3 cases of skin actino, also cured; and Koettnitz, 3 more cases of lump jaw in man permanently cured. Ponfick, in 1882, records 16 cases, 8 of which were cured. Fifty per cent. cured. Think of that, ye contagio infectionists! The above cases are certainly numerous enough to show that actinomycosis cannot be classed as an infectious disease in any sense of the word, for the annals of medicine in all countries and all ages have no record of an infectious disease of any kind, acute or chronic, which can be cured by an operation in the above manner. It is clear to any one who will read the records of actinomycosis in human beings that not only is the disease curable, but that it is so in every case where the lesions can be removed and treated in a purely surgical manner. Even very severe cases of abdominal actino are now yielding to the skill of surgeons, and with the continual advance of that art it is very certain that the cases of "cure" will be materially increased, and that the only incurable ones will be those in which the lesions are so situated that surgery dare not interfere. A Dr. Murphy, of Chicago, is (incorrectly) reported by the Illinois Commission to have testified that "if it (actino) is in the jaw it is not curable there." Evidence enough has been advanced to show that such testimony as that is worthless, as every case of jaw actino can be cured by a skilful surgeon.

Another very interesting and instructive point can only be seen by a careful study of the statistics. It is this: that very few cases of actinomycosis have been found in butchers or meat dealers in comparison to the vast number in agriculturalists or farm laborers, or those having to do with grains or grasses; in fact, the greater number have been in this class of people.

On the point of actinomycosis being an infectious disease, Bostrom says: "I can only consider the changes produced by the actinomyces (the fungi) in the organism to be of a simple inflammatory character, and the products as only those of simple inflammation. The tissues produced by the action of the fungus I hold to be of a transitory character, which opinion is supported by their complete disappearance and healing, which often follows rupture of the abscess, as well as other reasons, which contradict the idea that these products should be classed with the genuine tumors. I must protest against the classification of actinomycosis with the infectious neoplasms.

What then is the nature of actinomycosis?

Actinomycosis is an invasive disease, that is a parasitic disease, in this case a vegetable parasite, which on an opening being present, either natural or accidental, can invade the tissue and develop there because the nutrition is favorable, but which of and by itself never produces a chemical irritant which, by absorption or otherwise, gains access to the blood and produces a general constitution disturbance or infection.

INVASIVE DISEASES SOMETIMES CONTAGIOUS, BUT NEVER INFECTIOUS.

The above statement at first sight appears to contradict a previous one, viz.: that while all contagious diseases are infectious, all infectious diseases are not by any means contagious. On the other hand, we must look for the evidence of the correctness of the statement in an entirely different direction; one in which many invasive diseases resemble those of an infectious character, but in which infection itself does not occur, viz.: transmissibility. It has been said already that from the clinical and experimental points of view that the word contagious is identical with transmissibility; that is, the specific cause can be transmitted from one animal to another, the nutrition in each case being favorable, and in the second produce disease, as in the first, the disease induced in the second animal, if of another species, need not necessarily present the same lesions as in the species in which the disease occurs as a natural affection. Invasive diseases are also of parasitic origin. These parasites are also obligatory or facultative in many cases. The obligatory invasive parasites which cause disease in animal life are all, I think, of an animal nature, while the facultative are limited to the vegetable kingdom. Those invasive diseases which are of an obligatory parasitic nature, are in many cases, but by no means all, contagious, in the primary origin sense. For instance, the various forms of itch, due to mites, are due to obligatory animal parasites, which only originate in or on, and frequently are limited to one species of animals, while others can be transmitted from the original species to others. These varieties of invasive diseases are transmissible, contagious, but here contagiousness, in the sense of many medical wonders of this century, is not always proven, in fact, is often contradicted, by the inability to transmit to rabbits and guinea-pigs, or other animals than those belonging to the original species. These great authorities would be justified, according to their reasoning in other cases, in asserting that the contagiousness was not proven, though contradicted by the daily experiences of the centuries. The itch mites of one species will seldom live in the skin of another; the intestinal worms, tapes, have their peculiar hosts, outside the limits of which they will not live. Cysticercus cellulosa, the measle of the hog is a small bladder embryo, if we take one from pork, and transfer it to our mouths and swallow it it becomes taenia solium; cysticercus bovis is a similar

embryo in beef, and if we do the same with it we derive taenia mediocanellata. In both cases we have transmitted a living organism, which can produce disease from the hog, or cattle, to ourselves, and yet no man but a medical professor would have the effrontery to assert that a contagion had been transmitted, or that the produced disease was contagious because of the transmission. Trichiniasis can be transmitted from hogs to man in the same way, but is not contagious. Thus it is to be seen that in considering the real meaning of the words contagious or transmission, that they may have one significance in one place, and another in another, and none at all in other connections. In other words, we must first know what we are talking about. That something is the natural primary origin of the cause of a given disease, original home of its parasitic cause, and, again, its natural course between individuals.

These things have been all known for centuries, or must be definitely found out before we begin to think of the investigation of such diseases in the laboratory, where we can only study the individual characteristics of the parasites, their peculiar biological phenomena and morphological variations, but to make the results of such studies practical we must be able, by the study of the lessons produced in the animal body, to theoretically trace the life of the action of the germs therein, and, again, if exogenic germs, in the earth and animal surroundings. Laboratory investigations are but artificial aids by which to study the lives of disease-producing parasites in their natural habitat. Taken by themselves they may be strongly indicative, but must never be looked upon as conclusive evidence. The latter can only be obtained when through the former we arrive at a correct knowledge of the lives of such parasites in their natural habitats. The natural history of a disease, its origin and clinical course give us its character. The laboratory can establish its true cause and many points in the biology of the same.

REPORT ON AUTOPSY OF STATE FARM COW AFFECTED WITH JAW ACTINOMYCOSIS.

Thoroughbred Henford cow "Diana," eleven years old January 1, 1892; weight, 1,150 lbs. This cow has been more or less under my personal observation ever since I came to Nebraska in 1886, and to my personal knowledge has produced four splendid calves which are still healthy. The accompanying communication from the late foreman of the State farm shows that this cow has been affected with jaw actinomycosis for over five years, and that no other case has occurred on the farm. This fact alone suffices to contradict any assertion that actinomycosis is a dangerous transmissible disease between diseased and healthy cattle:

LINCOLN, NEB., January 2, 1892.

Dr. F. S. Billings.

DEAR SIR:—In answer to your question in regards to the Henford cow Diana (No. 2,997, A. H. R.). According to the farm record she was brought to the farm in 1882, and was there when I took charge in August, 1884, smooth and all right; but in the winter of 1884–85, she was hooked under the jaw by the cow standing next to her in the stable. Shortly after this I noticed a small lump growing on the jaw where she was hooked, and later on called a veterinarian to look at it. He called it "lump jaw," which it proved to be, in very bad form, twisting the jaw out of shape. This is the first and only case of lump jaw in the farm herd. There has been from twenty to fifty head of cattle in the herd from the time I took charge up to the present time, and the cow Diana has been continually with the herd in the pasture and in the feed lot, where we feed cow-fodder and hay to them. I have frequently seen matter discharging from the lump and drop on the ground and litter in the yard, and have seen this cow rubbing her jaw on the manger and edge of the water-trough. We never took any precaution to keep her away from the other cattle while the lump was discharging. She has dropped six calves, which are smooth and free from disease or any signs of lump jaw. Yours, very truly,

J. D. COURTNEY,
Late foreman Nebraska State Farm.

ILLUSTRATIONS.

The above illustration presents the condition of the "lump" on his cow's jaw far better than any pen can describe it. The general physical condition has been as perfect as could be asked for, notwithstanding the malposition of her jaws due to the atrophy of the masseter muscles on her affected side, and consequent contraction of the other. She had become "cross-billed," as we say of fowls. The position of the jaw was such, so much distorted on one side, that the mouth did not close perfectly, and in the process of rumination, more or less of her food would fall out on the ground, and become mixed with the food or fodder of the other cattle. I have frequently seen the discharge from the lump, which always had several fistulous openings in it, drop on the ground and on the corn fodder which the cattle were eating, and when she went to drink she would rub the tumefaction on the edge of the trough and often press out matter, not only on the edge, but it would run down in the water, to which all the cattle had ascess in common. The enlargement itself was fully of the dimensions of a half-peck measure trough; I am sure it had grown smaller during the last year, a fact also noticed by Mr. Perrie, the present foreman of the farm. At the time he killed this cow, January 15, 1892, there was one fistulous opening still discharging a thick stream of yellow material. The scarcity of actinomycosis clusters in this material, as well as that filling pockets in her jaw, was a matter of great astonishment to me, especially when taken in consideration with the enormous extent of the external lesions. This fact, however, in all probability, accounts for the general diminishing of said lesions in extent during the past year, as mentioned above. The left jaw was the one complicated. The animal was knocked on the head and carefully dressed by a butcher in my presence and that of my assistant, Dr. F. W. Brewer, Dr. Griffin, one of the leading practitioners of Lincoln, and several other gentlemen.

NECROSCOPY.

On removing the skin, all who were present were at once struck with the remarkable development of the subcutaneous fat tissue and the amount lining and covering those parts usually so lean at the abdominal cavity. Under the most exacting conditions the amount would have to be designated as a "fat" one. The cow was cut all to pieces, and every muscle and every organ and all the lymph glands most carefully examined, the same being afterwards subjected to a most exacting and detailed microscopic examination. Sections of those organs and muscles have been and are continually used in the laboratory for microscopic study, and yet not an iota of a lesion of any kind has been found in any of them. The muscles and organs were also subjected to most exacting microscopic examinations immediately after slaughter, which extended over several hours, and before any post-mortem changes could occur, but in no instance could we find any pathological granulations, or any lesion of the protoplasm in the cells or fibers. I can honestly and most emphatically affirm that if one hundred to one thousand of the choicest short-horn breed in the country were subjected to such an examination, every

one of them having been in absolutely perfect health up to the time of slaughter, whether yearling, two or three years old or aged cattle, that not a single animal could possibly present a more perfect picture of absolute health in all its organs and muscles (except those of the immediately afflicted parts) than did this cow, even though she had had actinomycosis for five years. Aside from the tissues of and around the complicated jaw, not even a microscopic speck of disease had been discovered in any organ or tissue, with the exception of two small urine-retention cysts about the size of a pea in one kidney. Not an indication of feverish meat could be seen anywhere.

My assistant, Dr. Brewer, and friend, Dr. Griffin (both M.Ds.), each took one of the tenderloins home and consumed them in their families, as is attested in the following letters. I should have done the same myself, but living in a hotel it would have been next to impossible to know whether I actually got it or not; in fact, I could not assert the fact that was certainly necessary in such a case.

LINCOLN, April 8, 1892.

I hereby certify that I was present at the Experiment Station Farm on January 5, 1892, when an autopsy was made on the cow "Diana," which had been suffering from lump jaw, apart from the protuberance on her jaw and the adjacent tissues there were not any symptoms of disease apparent in other parts of her carcass. I took home a large portion of one of her tenderloins. This tenderloin was eaten by my wife, my wife's sister, and myself, and there were not, and has not been, any unpleasant sensation, or any symptom of the slightest character to indicate that the food was at all deleterious.

F. W. BREWER, M.D.

LINCOLN, NEB., April 8, 1892.

I saw the autopsy of the cow "Diana" mentioned by Dr. Billings, and examined the muscles and tissues, as well as took in the microscopic examination, and finding nothing abnormal in the same, took a tenderloin home with me; of this I ate freely several times without any unpleasant symptoms, three months now having elapsed since eating it.

R. E. GRIFFIN, M.D.

That "fever" lesions should not have been expected in this animal is evident by referring to the previously given table of temperatures, taken for a period of two weeks prior to slaughtering; as a matter of convenience that table is again produced here:

	Shorthorn Cow Calved January 18, 1885.				Shorthorn Heifer Calved Jan. 10, 1889.				Lumpjaw Cow Born Jan 7, 1881.			
Dec. 1.....	A.M.	38.80	P.M.	39.20	A.M.	39.50	P.M.	39.30	A.M.	39.30	P.M.	39.10
" 2.....	"	——	"	39.10	"	——	"	39.10	"	——	"	39.10
" 3.....	"	38.40	"	38.40	"	39.50	"	39.40	"	39.10	"	39.30
" 4.....	"	39.10	"	38.65	"	39.80	"	39.30	"	39.20	"	39.00
" 5.....	"	39.30	"	38.80	"	39.70	"	39.80	"	39.20	"	39.10
" 6.....	"	38.70	"	39.00	"	39.70	"	39.60	"	39.20	"	39.15
" 7.....	"	38.60	"	38.70	"	39.90	"	39.50	"	38.90	"	38.70
" 8.....	"	38.90	"	38.80	"	39.90	"	39.50	"	39.10	"	39.20
" 9.....	"	38.70	"	39.00	"	39.50	"	40.50	"	39.00	"	39.20
" 10.....	"	38.90	"	38.90	"	39.70	"	40.00	"	39.20	"	39.80
" 11.....	"	38.90	"	39.00	"	39.60	"	39.80	"	39.05	"	39.10
" 12.....	"	38.90	"	40.80	"	39.90	"	40.10	"	38.90	"	39.30
" 13.....	"	39.10	"	39.00	"	39.90	"	39.95	"	39.00	"	39.10
" 14.....	"	39.00	"	——	"	40.00	"	——	"	39.20	"	——

Attention has been previously called to the astonishing scarcity of the clusters of actinomycoses in the matter generally called pus, which exuded from a fistulous opening, and was present in quite a number of small pockets in the jaw itself. That this material was not pus, and that in general (in cattle at least), it is not pus, is shown in the result of the attempt to make cultures from the same; one hundred tubes representing glycerine-agar, glycerine-agar plus formate of soda, blood-serum, and twenty-four glycerine-bullion flasks were inoculated from this material taken from a pocket in the jaw; all but six remain absolutely sterile; these six were pure cultivation of actinomyses. These tubes were placed in the thermostat at 37° C, about half in Buckner's cancerous mixture in hermetically sealed tubes. I must assert that the actinomyces will develop at room temperature, 70° F on blood-serum, but not on other media.

For us to-day pus can no longer be considered any yellowish pus-like discharge. To be pus such material must contain pus-producing germs, and they must be demonstrated to be present in the material, both microscopically and by cultures.

Cover glass specimens of this material also gave negative results as to cocci on any suspicious forms of germ life, other than a few clusters of actinomyses and some fragmentary development of the same. The inoculation of animals in the ear and subcutaneous also gave negative results as to pus-producing germs. These results have a most important bearing upon that all-important question, the exclusion or not of the flesh of such animals for consumption. The fact that no danger to consumer has ever resulted confirms the result in this examination, and that the actinomyces do not produce any irritable or dangerous chemical products, and hence have no deleterious effect on the general organism.

My cultures correspond with most of the illustrations given by Max Wolf and James Israel in Virchow's Archives, Vol. CXXVI, p. 11, for the same cultivation-media (I did not use egg), and as I am not engaged in the reputation of other people's work and have no especial interest in bacterio-morphology when the points have become fairly well established. I refer those interested in the above to Bostroem's publication in Zeigler's *Beiträge zur pathologischen Anatomie*, Vol. IX, 1891, as I shall publish nothing further on the morphology of this fungus and its pathology, as that which has a pathogenic interest on plants is a matter for botanical investigation and not for a pathologist in animal tissues only.

To return to our cow:

The skin covering her tumefaction presented quite a number of cicatricial contractions, marking the closed opening of former fistula; it was closely attached or united to her underlying tissue, which was thickened and indurated, being from one to two inches thick in many places. Another surprising fact was that in this enormous mass of pendulant tissue (as large as a half pint measure) not a single pus center could be found, or one containing the previously noticed straw colored material. The tumefaction consisted mostly of adipose tissue separated into sections of various dimensions by close septa of white connective tissue. The opening of the fistula, out of which exuded the previously mentioned odorless straw-yellowish material, was in direct connection with a pap or canal having close, white, thick connective tissue walls, which directly connected with a deep-seated pocket in the jaw filled with the same material, several such cords could be traced extending through the mass of neoplastic tissue from the closed openings previously mentioned. The fat was bright yellow in color. Immediately attached to the jaw and in the thickened tissue were numerous such centers, of a delicate pinkish color, interrupted by large numbers of pin head spots of a yellow color. Sections of this tissue gave most beautiful specimens of actively invading actinomyses, all illustrated by Bostroem. On the external surface of the complicated jaw was a marked projection and cell, indicating that the jaw had been fractured before the cow had been hooked, as indicated previously; the internal tissues of the jaw were spongy, with many centers of a semi-gelatinous character; pockets filled with the non-odor pus-like material (and much more viscid than pus usually is),

really having more of a thick mucous character, more outlined here and there, and now quite small and now longer than a half segment of a dove's egg. The parotid gland on the opposite side was indurated somewhat, but otherwise free from complications; all the lymph glands in the vicinity were entirely free of any actinomycolic complications. A few were atrophied or indurated from pressure, but the others were entirely free. Tongue normal.

IS THE FLESH OF LUMP-JAWED CATTLE TO BE CONDEMNED AS UNSUITABLE FOR HUMAN FOOD?

What is diseased meat?

Were that question to be answered in the manner the language indicates, it would not be correct. Practically expressed, the question which should be asked is, What must or may be considered disease producing meat, that is, flesh which should be withheld from sale and condemned as unfit for human food?

It may not be generally known to the public, and probably many physicians have not given any real thought to the subject, that all meat from diseased animals, even from animals having a notoriously dangerous contagious disease, is not considered as "diseased meat," when the animal is in otherwise good condition. For instance, thousands of pounds of meat from cattle diseased with contagious pleuro-pneumonia were allowed, even by this very Illinois Live Stock Commission, to be sold in Chicago, when that disease prevailed there. Mind, I do not even insinuate any carelessness on the part of that Commission in permitting its sale. They were right in doing so—when the general and special condition of the animal warranted it. In such cases, as in actino, a special examination of each carcass must be invariably made. In all European countries the same freedom of sale of the flesh of cattle having this disease, pleuro-pneumonia, is permitted, always dependent upon the nature of the lesions. On the other hand, if that flesh is subjected to either a microscopic or macroscopical examination, there is not a pathologist in the world who would say that the flesh was not "diseased flesh." If we turn to one of the highest and perhaps most rigid authorities on this question of what meat is fit to eat, Schmidt, Muelhim, Handbuch der Fleischkunde, p. 202, we shall find that the flesh from cattle having contagious pleuro-pneumonia, is handled very briefly, only fifteen lines being given to it.

He says: The contagious pleuro-pneumonia of cattle is an exceedingly chronic infectious disease. The flesh of slaughtered animals has been consumed in many thousands of cases without any harm whatever resulting, even when the animal was in a condition of fully developed fever. The meat inspector must, however, carefully look out for gangrenous centers in the lungs, which often lead to septicæmia. In such cases the meat should be excluded from the market.

Baillet, another world-wide authority, in his Traite de l' inspection des Viandes de Boucherie, says: "All practitioners admit, with common accord, that the usage of the flesh of animals attacked with contagious pleuro-pneumonia can be authorized without restriction. It has been established at the city of Lille alone that during a period of nineteen years, the consumption of such flesh was not followed by a single accident. In a period 18,000 diseased cows were slaughtered and their flesh freely consumed."

In this country thousands, one might be justified in saying millions, of hogs, diseased with swine plague, have been slaughtered and their flesh sold on the markets not only of this country, but the world, and yet not one single person is known to have become ill from it. Yet when we who know the nature of this disease (that it is a specific blood-poison septicæmia, and have seen the terrible lesions in the lungs and intestines), and see these things, the first and most natural sentiment is of the criminality of selling and slaughtering such hogs for human consumption. I must own that before this actinomycosis trial caused me to give special thought to the matter, that no man more mercilessly condemned the criminality of this practice, though I had probably eaten much such pork, and dispensed hundreds of pounds of it to sailors during my life as a seaman. Even though I had injected quite a dose of the bacilli of swine plague into my arm, and seen that no constitutional disturbances resulted, only a local swelling following which soon subsided, still my continuous investigations of the disease kept me of the same opinion. Too much of the general condemnation of persons, as well as other things, is based upon really unreflecting sentiment, which is a most dangerous and unjust practice. We like that which we think we like, and only too often refuse to try things, even people, which it would be very beneficial to us to have a far more intimate acquaintance with. Sentiment—feeling—is all right in its place, but never should be permitted to control our actions or expressions. Sentiment is a psychological phenomenon. Its physiological purpose is to call reflection into action. Only the collected and intelligent use of the latter should guide our actions or conclusions.

With regard to the personal relation of sentiment to ourselves, the results may not always be so important, though the following of our sentiment may be of gigantic influence to our whole future life. The divorce courts prove the correctness of that assertion. With regard to less æsthetic and more material things sentiment leads us also fearfully astray, or is liable to. It is a sentiment with me that Limburger cheese is the vilest stuff on earth, but my good wife and thousands of others, think it one of the greatest and sweetest luxuries. Stop my nose up and I can eat it, and the after taste is anything but disagreeable. To observe an autopsy on a dead person, particularly a decayed corpse rescued from the water, is most offensive to most people, yet to the true pathologist there is nothing disgusting about it. I would much prefer to use such a corpse for a table, and eat my meal while making an autopsy than to eat a piece of Limburger cheese. Many Europeans, as well as some Americans, are very fond of "over ripe" game, while the odor of one of those savory dishes, if carried by me to the expectant gourmand in a restaurant, would almost ruin my own meal. The first thought is, "what a disgusting appetite such a person has." We have to exercise "cold-blooded" reflection to overcome these prejudices of sentiment. We may not do that which pleases others, but should not condemn them for pleasing themselves, providing they injure no one else in doing so.

Sentiment too largely rules in regard to the action of authorities in many things. The sentiment of many people would certainly be against the usage of the flesh from pleuro-pneumonia diseased cattle, or swine plague hogs, but experience has completely demonstrated that sentiment is wrong; hence, cold blooded reflection and the law must of necessity ignore the sentiment. Sentiment is invariably the result of unreflection. Reflection has nothing to do with it. Sentiment assumes to know, but never

knows. We have a sentiment in this country that we should be grateful to France because she helped us in the Revolution, when cold-blooded reflection should have long ago taught us that we owe France nothing, for while she regarded us not at all, she hated England like the devil (it was mutual between them). Why should we respect action born of the most inhuman of sentiments, and carried out with cold-blooded vindictiveness. We formerly spoke gratefully of Germany, because her people took our bonds for their own benefit, acting on their best judgment, during the time of national trial, later on we damned her because she would not take our pork, though she claimed, for political reasons, that it was to prevent trichinosis in her own people. To-day, after she has taken off her trichina restrictions we still condemn her because she has put a big duty on our pork, forgetting that we can "see her better," by means of that beastly disgraceful and inhuman act, the McKinley bill. When we were boys, we were educated to hate John Bull because he taxed our fore-dad's tea, to-day we should hate ourselves for letting others tax us for similar reasons, to serve themselves. Old John was as right as our dads were. It was a question of might only, as it is now, who should tax the tea. The dads won the right to tax themselves when they pleased as with their own good might, as our sons and daughters will (by intellectual might) not to be taxed by unprincipled selfishness in years to come. Commerce has taught us that our sentimental hatred of John Bull was ridiculous, and experience has shown us that he is just as good a fellow as we are, with no more disagreeable peculiarities. We shall not dislike him again until he wins the "American Cup," then we shall put good reflection to work and build a boat to take it back. Sentiment creates the rivalry, but reflection and experience demonstrate that it is advantageous to the naval architecture of the world. It has been made evident that sentiment alone must not rule, and this is as true with regard to the "diseased meat" question as anywhere else, for in the cases quoted it can be seen that should it be followed an almost incalculable amount of good nourishing food would be withheld from the people.

The strange sentimental and illogical argument is frequently brought up that we have such a surplus of flesh products that there is no need that anything but the unquestionably best and lily-like purest should be allowed to be sold. With a million people more or less frequently out of work, and several millions more scarcely earning enough to keep body and soul together, such an argument falls to the ground by its own absurdity.

What if a lump jawed steer does present an unsightly appearance to the eye, so does maggotty cheese, the practical question is, Is there anything in the flesh of the animal which is actually dangerous to the public health?

If not, there is no reason which can be brought forward for the wholesale condemnation of the flesh of lump jawed cattle. Public sentiment has nothing to do with it. The public, as such, knows nothing on such questions. It has no more brains than a flock of sheep, and can be as easily stampeded by any wolf in sheep's clothing, who for self-interest starts any sensational bugbear. Every question of a public nature must be considered and treated in a just manner to all. This is as true of actino diseased cattle as anything else. If the balance of the carcass is in extra condition, that beef is extra quality, and so on with every grade. We grade beef now according to its quality, but, as has been shown elsewhere, tapeworm cysts are not looked for—not always according to its dangerous quality after all. Meat inspectors should be educated for the business, and know more of the relation of conditions dangerous to the public health in meat than any ordinary medical man. It is a highly scientific position as well as an eminently practical one. Laymen should have nothing to do with it. The placing of political heelers in such positions is an insult to the name of honest intelligence, and a reflection upon the public service of the country. Such positions demand the highest possible technical education, the most stubborn honesty, and above all that enthusiasm to serve the public, all sides of it, with intelligent, active fidelity. No man who wants such a position because he can scarcely make a living in his profession, or gets it because he can pull votes, should be in it for a moment.

Mind you it has been clearly shown that meat diseased in itself as in the case of pleuro pneumonia, swine plague and even the majority of Texas cattle, is not necessarily disease producing when used as food by our own species. On the other hand, it can be clearly shown that flesh, which to the eye, even of the expert butcher, is absolutely healthy in appearance can lodge an element which is most dangerous to us as food. Trichinous pork is alluded to. No condemnation, and so far as I know, no examination either, is made in this country for the presence of embryonal cysts of the armed and unarmed tape worms in pork and beef respectively; the flesh is absolutely normal, and yet both these parasites are often dangerous and always troublesome customers to have in one's intestines. A man may almost die of trichinosis to-day and five years from now be perfectly healthy, so that the sharpest physical examination and his own actions and true statement would fail to discover a sign of illness about him. He might poison himself, or be murdered, and yet the most exact optical consideration of his muscles—not microscopic—would fail in discovering an iota of evidence of disease, but if a cat or dog were given a piece of the same in the laboratory, most serious results to the animal would follow.

It is thus to be seen that this question as to what is diseased meat in reference to the public health is a most delicate one, and one not to be decided by ignorance, prejudicial sentiments or ignorant politicians.

With regard to actinomycosis in cattle, the striking facts were brought out at the late trial in Peoria, that not one of the so called "experts" for the Illinois Live Stock Commission had ever microscopically, and scarcely macroscopically, examined the flesh of any of the condemned animals, or of any of those parts of the body which under ordinary circumstances would naturally be sold in the market. I mean the muscular parts, back of the neck, including the anterior limbs, back, tenderloin and rump.

Yet these men were placed on the stand as experts, and accepted by the court.

WHAT IS A MEDICAL EXPERT?

That is not such an easy question to answer correctly as may at first appear. Every practitioner should be competent as an "expert" in all questions having to do with the practice of medicine; every surgeon should be expert in his line, but need not be and generally does not consider himself to be, such on many questions of general practice; all specialists should be expert in their particular line, while it is generally admitted that they ignore every other branch of medicine, or treat them very step-motherly.

Not one in a hundred American physicians knows anything expert in microscopy, and not even that percentage have any expert ideas of the principles of pathology. As to the veterinarians, the case is naturally still worse; in the majority of the so-called veterinary colleges of this country, microscopy is scarcely taught, and as far as pathology in the scientific sense is concerned, it is impossible under existing conditions to more than introduce the rudiments. Of the "experts" who appeared on the stand at Peoria, as veterinarians for the Commission, I will be bold enough to say what I personally know, that not one of them knows the methods of applying a microscope to pathological investigations; not one of them knows how to make a proper autopsy and correctly interpret the lesions and their relations to one another; not one of them has any practical knowledge of the methods of scientific investigation; in fact, not one of them is an expert on the subject of infectious diseases in any sense of the word.

These statements may read hard to my colleagues, but if so they have to blame themselves for putting themselves in a position they one and all were and are totally incompetent to fill. They know and I know, that it is not their fault that they are not more expert than they are in these matters. It is more the fault of the country than the experts, that the majority of them are totally ignorant on scientific questions. The country has never yet demanded that its physicians and veterinarians should be educated as every reputable physician should be, nor does it provide colleges where those who would educate themselves can find the proper opportunity. These men are competent, every day practical men; on subjects of that kind they are or should be experts, while I, who claim to know a little on the scientific side, am about as unscientific an individual on the practical side as can be found, scarcely knowing, except in a general way, how medicines act, and having still less knowledge and no experience as to their suitable application. We have too much "cheek," when a dollar is in sight in this country. Most men are expert enough to try and get that. It is no expert trick to recognize a cluster of the actino fungus under the microscope. But he would be quite an expert who could tell the germ when growing, or in its involutional forms unless he had had much practical experience. Had I been willing to lower myself to an unprofessional trick, and were it the custom in court, all these gentlemen could have been floored by not being able to recognize photographs of the development forms of the fungus.

In the bulletin of the Live Stock Commission on the late trial, the names of every "expert" they presented either in person or by deposition is given, sixteen in all if I counted right. Then to show the weakness of their position, the Commission strings out an attachment to each name of all the titles, positions, and probable hopes of each man. It's a wonder they did not put down to what church they belonged! This wonderful array of titles need not impose on any body. They are harmless and weightless. Position does not make the man, but the man honors and makes the position. Of those sixteen so-called authorities, seven may have some knowledge in the use of the microscope, while nine have no claims to any. As to authority on actinomycosis or infectious diseases scientifically, I do not know of one of them ever having done a thing or published anything which the world recognizes as entitling him to any such honor, and I do not exclude "the head and front" of the veterinary profession in the United States either. Heaven pity the tail if the "head and front" is to be judged by the "crucial tests" of its position on infectious diseases.

Here is a sample of the imbecilic drivelings of this "head and front of the Veterinary Profession in the United States."

"That the disease is contagious is self evident, since it depends on the planting of the germ in the animal tissues, affecting, as it so frequently does, the region of the face, mouth and tongue; that the disease is dangerous is unquestionable, as in the absence of treatment it usually goes on to a fatal issue, by the interference of the feeding and mastication, or by the extension of the malady to vital internal organs."

Just read that and think over it! An infectious disease that "usually goes on in the absence of treatment." Is there one that does not go on, treatment or not? The veriest cow-doctor in the country has more common sense than to put forth such trash as the above—bah! on such authorities! A profession with such a head and front as that had better resurrect itself at once or it will die of dead rot. It must be very evident that a vast number of otherwise reputable medical writers are in a most illogical position regarding the only use in which this word contagious can possibly be correctly applied. 'Tis true that to use it in its own root-sense, it is identical with transmission, but it is equally true that in that sense their entire fabric collapses from its own weakness—for, as said before, both contagious and infectious diseases are transmissible, then where comes the universally admitted differentiation that "while all contagious diseases are infectious, by no means all infectious diseases are contagious." According to all these writers, and these "head and tail authorities of the Commission," there is no difference because all can be transmitted; hence all must be contagious.

These writers should be known as the "contactionists."

"FOR THE LAST SHALL BE FIRST AND THE FIRST LAST."

It is hard on the Illinois Live Stock Commission and their so-called authorities, especially the "head and front" of the Veterinary Profession that the facts ordain that they must take the place at the "tail of the procession." The verdict all over the world is against them, as may be seen from the following, in addition to all the evidence given in these papers:

"PHILADELPHIA, January 27.

"The result of a special investigation by the doctors of the University Veterinary School on the subject of lumpy jaw, common to cattle, is made public. They find no case on record where the disease was transmitted to a man from eating diseased meat. The conclusion is reached, therefore, that where the liver and other organs are found not to be affected, it is probably safe to use the meat, and they see no reason why the meat should not be sold after proper inspection."

To be an authority requires several things:

1. General and special education in a special branch and all affiliated branches.

2. Special work in that branch and affiliated branches.

3. Brains enough to weigh all the evidence and honesty enough to speak the truth, though the Heavens fall and hell opens its gates, and all the machine politicians in the country damn him to everlasting perdition.

This is a pathological question. How many authorities in that sense has the world had? One—

only one—sublime authority, the living and yet immortal Virchow. All others are but reflections of the greater light. On hygiene how many have we? More, but they are fitted men and honest ones—not half-educated veterinarians and position-seeking physicians. On actino we have a number, but scarcely one deserves that title in this country. I do not know of six men in the country whose general and special education, as pathologists, would entitle them to speak "expertly" on this question, and then only after they had reviewed all the testimony—and that is more of an undertaking than most experts may be aware of. As most of the testimony is in German, how many of the Commission's "experts" know anything about it? Having seen a case or two of actino in man, or seen butchers cut up even "six hundred" cases in cattle, does not necessarily warrant a person in being looked upon as an expert in the question. He may not know much more than the butchers, and, if prejudiced or purchased, know even less.

Millions of people know the way babes are made, but not one in a million knows how to bring one up, and no more than that how the little creatures are put together; of the wonderful involutions and evolutions of the germinal plates. Even in science the world produces many good observers, but the great authorities are pole stars in the immortal firmament. It has taken millions of years, billions of fornications, and millions of accidental conceptions, to brilliantize the firmament with an Aristotle, a Hippocrates, a Virchow, and a Darwin; and in other lines of science the stars are no more abundant.

As a frightful example of the expert ignorance of some of the experts referred to in this paper, they undertook to make a microscopic comparison between the diameter of the extremity, which is known as a "club" of the actino fungus, and a red blood cell, and finding that portion of the club (they said) less in diameter, asserted that it could pass through blood-vessels smaller than the red cell could. In the "bulletin" just issued by the Commission, they say that I expressed the opinion "that the germs were too large to travel by means of the circulation," or words to that effect. That is false! I knew what they did not know and may now learn: that in its course of active development the ray-fungus is not such at all, but develops as threads and short rods, or segments, as small as the swine-plague bacillus, and which can be carried by the circulation anywhere. The question now is not CAN they get into the circulation and flesh, *but do they?*

All practical experience shows that they do not, though in man alone several cases of penetration of blood-vessels have been reported, with actino in the heart, brain, etc., but not in the muscles of the trunk. Persons who testify recklessly as to things crawling round in the circulation, or being carried here and there, need to know a little more about the "things," as shown elsewhere, and also about the facts as reported. No case of actinomycoses—the fungus being found in the kidney—as conveyed by the circulation, is recorded in man or beast.

'Twould be well to think of that! The kidneys are in the direct line of the arterial circulation, and get the blood they work on direct from the great circulation. Do not forget that point. The extension "*per continuitatem*" of the fungus—per invasion—from an intestinal or other lesion, is not via the circulation.

The only case of actino in the liver which I have seen was sent me by the Commission. The sack, filled with a peculiar, viscid, yellow stuff that could not be called pus, was a distension of a gall duct and not a blood-vessel, as any competent person could easily see, and the fungus must have been carried into the duct from the intestines by some worm parasite; the connecting duct on each side of the "sack" was easily to be seen.

All cases of actino in the circulation, thus far reported in men have been on the venous side; this accounts for the immunity of the flesh at all places at all distant from the local lesion.

It is worthy of note that several pathologists mention the presence of trichinæ in the muscles of patients that have died of actino, but not the actino; this shows pretty careful examination. There is not a reliable report of a single autopsy in cattle which gives an iota of evidence of the fungus getting into the arterial circulation, the only way it could get to the flesh of the trunk, and I do not know of a case where it has been reported that it can be safely said the thing was carried by the veins even.

Those experts who thought that they did a great thing in guessing at the difference in the diameters between the point of a club of actino and a red blood cell, forgot several things:

1. That in the club form the fungus is absolutely dead and harmless, and that it is the thread does the invading!

2. That the red blood cell has no fixity of dimensions in active circulation, but is an elastic body capable of adopting itself to the finest capillaries, whereas the end of the club is a hard, unelastic object that could not possibly pass where the red cells can.

3. They forget that the club is not a round object, but a long one, and that its longitudinal diameter exceeds that of its point by a great many times; hence one end being heavier, it is very improbable that such an object could be carried very far in the circulation, it would get cross-ways at the first branching of medium-sized vessels, not to speak of capillaries.

4. They did not know, experts though they imagine themselves, that to measure one point of one club would give no correct idea, and that these club points frequently represent a common root, like a man's arm, with three or more clubs, like fingers, at the end, so that they should have isolated one of these sections, and there they would have found a stiff, unchangeable, sub-divided object as large as a dozen red blood cells.

Experts! Experts! Such ignorance is an insult to the very idea of technical education, and would not be put up with in any other profession than the semi-quack, two-thirds illiterate medical and veterinary professions of this blessed country, the home of fraud in nearly every high and official position. I want to say one word on authorities. When the authorities agree with facts and common sense, I agree with the authorities, otherwise they are not worthy a moment's consideration.

Nature is the only high authority, and the correct interpreter of her manifestations the second.

How many men have shown themselves worthy of even a place low down on the footstool of the immortal throne?

The trouble is in our courts and not the "experts." The bars should be drawn so sharply as to exclude all but the most competent.

This expert business in our courts is as big a farce as the jury system. If one knows anything, the court (I do not mean the judge) wants as little of it as possible; your own side hopes the cross-questioning will

bring it out and that side dare not. The biggest ignoramus with an M. D. attached to his name would stand on a par with a Virchow in an American court. The real expert cannot appear as such. No matter how much one may know the justice of his cause, he must be retained, and the impression on the jury is that it is the fee which rules his opinion. No expert testimony can ever have its real value in the courts of this country until the law is so changed that the real experts are summoned by the court and permitted to give their testimony according to existing knowledge, and to quote their authorities and place them before the court, in exactly the same manners as the lawyers do. Retained experts are a humbug; a disgrace to the intelligence of our courts and to justice.

THE USE OF THE FLESH OF ACTINOMYCOTIC ANIMALS SUFFERED NO RESTRICTION WHATEVER PRIOR TO THE YEAR 1877.

As to the flesh of cattle diseased with actinomycosis, it has been eaten freely for centuries in all parts of the world, and even to-day, with the exception of the malignantly ignorant assertion of the Illinois Live Stock Commission, and its tools, not a single authority can be found, not a single scientific and reliable physician who even harbors the suspicion that the disease in man has ever been caused, or will be found to be caused by eating the flesh of such cattle. It was but natural that such a suspicion should arise when the same fungus was found in the disease in both cattle and men, but experience has shown it to be groundless. The mere fact that the majority of the cases in human beings have been found in Germany, should not, as has been falsely intimated, be attributed to eating the meat of diseased cattle, but above all, to the general use of the microscope by German physicians, and again that the chances of exposure in human beings is one hundred to one in favor of this country, where harvesting is largely done by machinery, whereas in Germany it is done by hand, and then northern Europe may yet be found to have been the original or a favorable home for the parasite in vegetation. Prior to the year 1876—when Bollinger first described the disease in cattle, there was no suspicion that such meat could be dangerous. Since then the meat has been subjected to the most exacting microscopic examination in Europe, and in no single instance have either the lesions of diseased flesh been found, or anything resembling the fungus. Numerous cases of death from pulmonary, or other unoperable forms of actinomysis in man have been most exactly examined, and yet no investigator has reported a lesion in the muscles at any distance from the seat of the disease or the presence of the fungus in the same. For years this meat has been freely eaten in this country, and is knowingly so to-day, and yet not one single case of the disease in man can be traced to that flesh as a cause in this or any other country. Only when the Live Stock Commission of Illinois (and I regret to have to say it) found itself the honored victor over contagious pleuro-pneumonia and the same time to be out of a job—for what was there to do save an occasional case of glanders to be looked after, did we hear of this new and terribly dangerous "contagious disease" in cattle, "lump jaw" and that the meat was so dangerous that one scarcely dared look at it, far less eat it.

It is, to my mind, rather a serious undertaking for these men, who either do not, or pretend not to know what the verdict of the highest authorities on the relation of meat to the public health is upon this subject, to set themselves up as authorities upon a question in which, as has been shown, every one of their conclusions or statements is groundless and proven to be so by the admission of their own witnesses, and opposed by the stern logic of genetic and pathological facts.

WHAT THE REAL AUTHORITIES HAVE TO SAY!

Certainly the world recognizes no higher authority than that of Dr. Hertwig, Director General of the stock yards and slaughter houses of the city of Berlin. Long before this question ever got into the courts of this country, and even before it was definitely known that it would, I opposed the position taken by the Illinois Live Stock Commission in the columns of the *Breeders Gazette*. Knowing Dr. Hertwig intimately, and I myself having been a student in Berlin during the heat of the actinomycosis excitement, and examined many hundred sections of such muscles, I wrote the doctor for his opinion, based as I knew it was upon so many years careful observation supported as it has been by that of that expert meat examiner, Dr. Dunker. He wrote me from Berlin, March 3, 1890:

My Much-honored Colleague:

In answer to your question as to the presence of actinomycosis in the true flesh of cattle (*Eigentlichen Fleisch der Rinde*) I must say that I have never seen them there. I have seen the fungus in the muscles of the head; those immediately in the vicinity of the tumor and attached to it, but never in the tenderloins—psoas—nor in the flesh sold for consumption. Yours truly,

HERTWIG.

And again, under date of "Berlin, April 6, 1891," he wrote me, giving the exact conditions, or regulations, which rule at Berlin in this questions, as follows:

1. We do not condemn any cattle which are attacked with jaw actinomycosis. Only the diseased head is condemned.
2. The case in which a whole beef would be condemned has only occurred once since inspection began here. In this case the head, tongue, liver, lungs, and lymph glands were all seriously invaded, and the body in a very emaciated condition.
4. So long as the trouble remains local, that is, confined to the head, and no general disturbances is present, we do not, and shall not, consider the meat of the balance of the body as unsuitable for consumption.

HERTWIG, *Director.*

It should be known that these regulations are not the result of the personal observations and conclusions of Hertwig alone, but more especially of the Imperial Board of Health, which has the most competent corps of investigators in the world at its command, and that they must also be sanctioned by the German parliament.

A certain witness, Dr. Hewitt, of Minnesota, brought forward by the Commission, in his conversation and partially in his testimony, endeavored to show that meat of actinomycosis—lump-jaw—cattle was permitted to be sold in Germany, because of the scarcity of animal food there in proportion to the population. Such an argument as that is based on utter ignorance of the principles and actions of the German government. 'Tis true, that sentiment outside of patriotism, plays a very insignificant rôle in the rulings of that government in comparison to the unintelligent gust which prevails in this country and England on many such questions, and it is equally true, that while we gush all over about brotherly love and all that nonsense, that as a government and a people collectively the health of a human being in this country has no value whatever, nor does his animal

property either, unless gush can pull votes, while in Germany, the public health and wealth are watched over with even more than paternal anxiety, for every man and every woman there either represents a defender of the country or the producer of one. Recklessly as the Government of Germany exposes its male citizens to slaughter in its wars, it as carefully looks after their health and prosperity in time of peace. It is said of the poet Heine, that when asked "what place the German working classes occupied in national economy," that he answered it was to "fornicate, fornicate, fornicate, in order to supply plenty of food for French cannon." The food produced, the government takes the very best care of it, while here and in England, it has not as much value in the eye of the government, or the public, as a "beef critter or a hog.

You don't like this, my countrymen. Truth is frequently a bitter pill to swallow. Let us see if these charges are not founded on fact? Where is the laboratory in this country under government support studying the diseases of our own species and seeking means to protect our health and lives? How many has Germany?

All the necessities of the country require, and other European governments are not far behind, because man has an economical and national preservational importance. Even the recent political examination of our meat products is for foreign consumption, and to catch the farmer's votes, and not for our own use or benefit. The law says, "if unsuitable for export to be handed back to the butchers to be used according to local laws," or words to that effect. It can be even extended to flesh destined to be exported to other States, but our local people must eat the refuse. For once, neither Congress nor Jerry Rusk is to blame, but this does not change the facts in the case.

These same laboratories in Europe are also busied in seeking means to preserve the live stock of the respective countries from disease that the people may prosper and have abundance of food.

In this country Nebraska stands alone in the present sisterhood of States, and even her energetic attempts meet with nothing but malicious opposition from the national government. Here, in Nebraska, the intelligence of the people demands that all possible shall be done to protect their agricultural interests, and that same intelligence will long ere demand that the investigations be extended to their personal interests, seeking for means to preserve their own lives and those of their children from disease.

One of the greatest feeders of cattle in Nebraska, as well as the world, Mr. Allen, manager of the Standard Cattle Company at Ames, where they feed five or six thousand head at a time, writes me "that he has never seen the disease extend among his cattle from those having the lump-jaw, and that since the seizure and condemnation of such cattle in the market, they slaughter all they have for home use, and have no compulsions about eating the meat, and that no ill effects have ever followed."

The greatest authorities in the world upon this question all coincide that the meat can be used with impunity if the diseased parts are thrown away, and that there are no indications of diseases in the marketable flesh itself.

Let us see what these authorities say:

There is a little volume entitled "Meat Inspection," written by Thomas Walley, M. R. C. V. S., Principal of the Edinburgh Royal (Dick's) Veterinary College, and Professor of Veterinary Medicine and Surgery. Before introducing the testimony of this witness a word as to his competency may be in order. In addition to a front rank among the veterinarians of Great Britain, an experience of thirty years in his profession and an official connection of over sixteen years with the Edinburgh Abbattoirs certainly qualify him to speak understandingly on this subject. This is his evidence ("Meat Inspection," pp. 128–129):

The malady affects man, and is in him known as actinomycosis hominis, and while it has been transmitted by inoculation from man to the calf (Crookshank), so far as I am aware there is no direct evidence of the transmission of the disease from animals to man, nor indeed is such a contingency ever likely to arise, seeing that the vitality of the spores of the hyphomycetes is much inferior to those of other pathogenetic fungi, and that the changes produced by it in the organs attacked are so marked as to attract the immediate attention of even ordinary persons; and what is of more importance, the lesions of the disease are seldom localized, except in the pig, in the muscular tissues of the body; there may, however, be lesions in the muscles immediately contiguous to or connected with affected organs. The spores of the disease are supposed to gain access to the structure of the tongue and other organs by the medium of a break of surface, such as a wound, an abrasion, or a crack, and after so gaining access, rapid development of the fungus goes on, while multiplication and dissemination of the spores take place. The irritation set up by the fungus leads to the production of a condition analogous to that seen in tuberculosis, viz., to a diffuse and chronic (interstitial) inflammation of the tissue of the organ, with the formation of nodules or granulations almost identical in morphological characters with those of tuberculosis. They are, however, larger than the miliary nodules of the latter disease, and do not, like them, tend to aggregate or coalesce, and consequently do not lead to the formation of such extensive lesions; and while calcification goes on in the center of the nodule (constituting its core) caseation is much less pronounced. As has already been indicated, the glossal disease may invade the lips, the cheeks, and even the muscles and bones of the jaws and face, producing material changes in these parts, such as the destruction of the cancellated portion of the bone, and opening up (rarefaction) of the compact tissue; softening of the new growth goes on with the formation of imperfect abscesses, the contents of which may be evacuated through cloacæ in the bone, and finally, by sinuses, through the soft tissues and the skin. The evacuation of this softened matter is followed by healing and the formation of a cicatrix; hence the scars so frequently seen on the face and cheeks of affected animals. The internal organs may become invaded, primarily, through the medium of the various mucous membranes; but the theory has been promulgated that a localized lesion may become, by virtue of the infectivity of the disease, systemically disseminated. Whether this is so or not is a matter of very little importance so far as our present purpose is concerned. In pigs, according to Johne, the actinomyces is frequently found in the tonsilar cavities, which may or may not have become the seat of any pathological change. Although certain organs, such as the tongue may be largely involved, there is not much accompanying fever or interference with the normal functions or with the nutrition of the blood, and any systemic changes that arise are due mainly to annihilation of the functions of the tongue or of the particular organ involved, and as a result of this a state of poverty is induced. Seeing that the disease

is rarely of a systemic nature, and that there is an absence of fever, the nutritive changes induced in the muscles are so slight as to do away, in the great majority of cases, with the question of nocuity of the flesh; but notwithstanding this, if there is any evidence of malnutrition of the blood or of the flesh, the carcass should be condemned for human food, and in every instance the affected organ or organs should be effectually destroyed.

VIEWS EXPRESSED AT THE INTERNATIONAL CONGRESS OF HYGIENE AT LONDON, AUGUST, 1891.

Lancet, Vol. II, pp. 444–5.

Actinomycosis was not really a new disease, but it was not recognized until comparatively recently as a specific micro-parasitic affection. There could be no doubt, from the writings of Professor Dick, that actinomycosis in cattle was extremely prevalent in Scotland from 1827 to 1839. In other parts of Great Britain it had probably been equally prevalent since, though lost sight of under various names, as wens, clyers, crewels, spina ventosa, carcinoma, etc. It was not until 1887, when Professor Crookshank established the etiology of wens, and investigated several outbreaks of a disease which proved to be actinomycosis, that it came to be generally recognized that actinomycosis in cattle was in some counties one of cattle in this country, and more recently the disease had been found to be equally prevalent in our Australian colonies and in the United States. Cases of actinomycosis in the human subject were met with at an early date. So long ago as 1848, M. Louis had a patient with pulmonary disease, supposed to be cancerous, and in pus from this case M. Lebert, of Zurich, discovered and accurately figured the tufts of club-shaped elements with which we are now so familiar. In late years nearly two hundred cases had been described in the human subject, of which over 5 per cent. occurred in this country. Professor Crookshank thought there were two points especially worthy of discussion. In the first place, some manifestations of actinomycosis were still liable to be mistaken for tuberculosis. How far would this account for the very high percentage of cases of tuberculosis in cattle, which had been reported by some observers? The question also arose, whether cases of actinomycosis ought to be placed in the same category as cases of tuberculosis, and condemned as unfit for food. If not, the distinction between the two diseases became a matter of practical importance, and the close similarity between the two diseases must be brought home to the minds of both medical and veterinary inspectors. With regard to inter-communicability of the disease between different species of animals, including man, the author had failed to find any evidence in support of the theory from clinical observation. He was not of the opinion that the disease was contagious in the ordinary sense of the term.

Professor Ponfick (Breslau) said that he agreed in the main with Professor Crookshank's conclusions. In cattle the disease was generally transmitted by means of the fodder, especially by straw. He thought that in both man and animals the source was the same, and that the disease did not spread directly from one to the other. Professor Nocard (Alfort) said that the observations of Professor Crookshank had been confirmed by all observers. He desired to dwell on the hygienic and sanitary side of the question. The geographical distribution of the disease was very irregular. Bavaria, Scotland, Italy, and some of the Northern States of America, had been much affected, and at the Veterinary Hospital at Utrecht there were always cases of the disease. In France it was very rare, and generally remained in an isolated form. He had never seen cases transmitted by contagion or infection. The disease appeared to him undoubtedly to spread by means of certain food-stuffs. The wholesale destruction of the flesh of the affected beasts appeared to him to be quite unnecessary. It was toward the biology of the parasite that the hygienist's efforts should be directed, that some rational and efficient mode of prophylaxis might be arrived at.

M. Doyen (Rheims) read a paper on three cases of actinomycosis in man, with lantern demonstration. During the last few months these observers have had occasion to examine, in man, three cases of actinomycosis, which in France, they say, is a very rare affection. If Liebert's case (1887) is excepted, the first observation was by Nocard and Leucet, in 1888. Israel had, however, published three cases, and Ponfick one, and numerous other cases have been recorded in Germany, before attention was called to the disease in France, since 1882. The authors had been on the outlook for the disease, but it was not until February, 1891, that they had the opportunity of observing their first two cases. The first patient consulted them for an indolent abscess of the jaw, with induration of the parotid. The cavity contained a bloody, serous fluid, in which were floating a dozen or more grains of actinomycosis; the whole mass was completely extirpated. The second case manifested itself as a deep, phlegmonous induration at the base of the tongue, accompanied by a wooden-like hardness of the suprahyoid region. This case was cured by free incision, followed by curetting and antiseptic tampons.

"As regards the etiology, these three cases all came from the environs of Rheims; they lived in the country, and one of them had acquired a habit of chewing grains of oats and of barley."

"Dr. Serge Ivanov (Moscow) had been to many of the European abattoirs to study this affection. In discharge of his duties as chief inspector of the Moscow meat market, he had seen over two thousand infected animals in the last two years. Most of these cases had been diagnosed in an early stage by the use of the microscope. Without this precaution, lesions in the shape of small tumors and abscesses might easily escape recognition. Dr. Goodall (Christchurch) had seen many cases of this affection, and he did not think there was sufficient evidence to justify the belief that the disease was communicable from animal to animal. It was worthy of note that it most commonly affected animals when they were about two years old and were changing their molar teeth. In one case he found a piece of heather firmly wedged between the teeth, and it was easy to see if such a structure were charged with the organism, how it might easily penetrate into the socket of the jaw beside the loosening tooth. Our present knowledge seemed to indicate that this was a fungus, which found its normal habitat in vegetable structures, and was only accidental in animals. He felt sure that a careful investigation of the diseases communicable from the vegetable to the animal world would be productive of very fruitful results. Sir Henry Simpson remarked that in his district the disease was not endemic, and when imported though it did not cease with the animals imported, yet it did not spread rapidly like a contagious disease. As to whether the meat from these carcasses was fit for human consumption, he was content to permit its sale provided the locally diseased parts were cut away. Dr. George Fleming

commented on the grave doubts which existed of the direct communicability of this disease to the human species. He thought that the legislature should not be overburdened in dealing with this matter, which could be controlled by ordinary measures. If any animal were in good condition and showed no fever, then its flesh was good to eat, but the actually diseased parts should not go into market."

Here then, we have placed all the necessary evidence before our readers. A careful perual of the same must have shown:

1. That actinomycosis is not, genetically, a contagious disease, being facultative, parasitic, or exogenetic, in origin, developing on some kind of vegetation.

2. That it is not an infectious disease, because:

a. When uncomplicated it runs it course without fever.

b. It can be cured by a surgical operation every time that its location is favorably situated in man or beast.

3. That transmission by inoculation has no necessary value in deciding the natural character or course of any disease as to its origin and relations in animals of the species in which it primarily occurs.

4. That no lesions, either macroscopical or microscopical have been found in any of the edible flesh at a point at any distance from the local center of invasion.

5. That the fungus itself has never been found in flesh distant from the lesion in man, notwithstanding the most exact examination.

6. That all persons who have any claim to be considered authorities agree in allowing that such flesh is suitable for human food, the diseased parts being removed, and the animal being otherwise in equal condition to a corresponding one without lump jaw affection.

7. That the disease is an invasive, facultative, parasitic disease, not producing any chemical irritant or poison, and, hence, incapable of producing any general constitutional disturbances.

8. That diseased meat does not necessarily include the flesh of diseased animals, but rather such flesh as contains chemical, or other elements, which of themselves are capable of producing disease in man when consumed as food.

In this regard the process of "ripening" as it is called, is on a parallel with disease, for as is well-known the "healthiest" meat when over-ripe is capable of causing the most serious disturbances in the human organism when eaten as food.

In this and the previous papers the full details and all the facts concerning actinomycosis in both man and beast have been placed before the reader, so far as they bear any relation whatever to the three interesting questions, viz.:

1. The contagiousness or not of the disease?

2. The infectiousness or not of the disease?

3. Whether the flesh is suitable for human food or not?

You can all form your conclusions, so that it is scarcely necessary to say that there is not a particle of evidence which can be adduced, or which even can be so twisted as to confirm either the conclusion or actions of the Illinois Live Stock Commissioners. It has been positively shown that

1. The disease is not contagious in origin.

2. That it is not infectious in character.

3. That the flesh is perfectly safe for human food when the animal, or carcass, is in otherwise marketable condition.

In the second paper of this series attention was pretty sharply called to the lamentable ignorance of the majority of the members of the medical profession as to the correct use of the technical terms proper to medicine. This ignorance is still greater on the part of the same individuals regarding the exact scientific status; that is, the true evidence, in just such questions as have been discussed here. While a very large number of American physicians are fine mechanical, rule of three practitioners, the majority of them are disgracefully ignorant as to the results of scientific research. Nowhere is this more true than in regard to the larger number of such persons who edit medical journals, most of which are not published by or for the benefit of the profession, but as means for publishers to advertise their wares. The majority of these editors are notoriously unqualified in all purely scientific matters, and often ignorantly prejudiced against the results of exact investigation. Many of them, more especially those editing journals issued by book publishing concerns, have been unsuccessful practitioners, who, knowing little, but assuming much, have gravitated into these "stool-pigeon" positions at small and dependent salaries. Their opinions on such questions as those here discussed are largely based on ignorance, and generally written on the side which is thought to be in sympathy with that dangerously misleading guide, public sentiment. An example of this kind of opinion recently appeared in an editorial in the *Medical News* of Philadelphia, a publication which has every advantage offered its editor to keep himself posted on matters of public importance, yet this editor never knew that the Congress of Hygiene met in London last August, and discussed the actinomycosis question, or knowing it, completely ignores the conclusions there presented. He is a fitting example of the cohesion between likes; for puffed up with his own ignorance he ignorantly supports the ignorance of the Illinois Live Stock Commission and its baseless assumptions. Any ordinary layman who carefully reads the following editorial, and has read the foregoing papers, will at once see that opinions, editorially expressed, are based on an entire ignorance of the facts, or ruled by the prejudice of sentiments which have a totally baseless foundation.

Read what he says:

"THE WHISKEY TRUST VERSUS THE LIVE STOCK COMMISSIONERS.

"The Live Stock Commissioners of Illinois have made themselves obnoxious to the Whiskey Trust by their efforts to prevent the sale of diseased cattle intended for the beef market in Chicago. Accordingly, the Trust brought suit against the Commissioners, in Peoria, which has ended in a disagreement of the jury. It would be made to appear that the farmers would suffer great loss, and that the cattle trade would be injured if the Commissioners were allowed to condemn beef cattle suffering from the communicable disease known as antinomycosis, or "lump-jaw." This is the same disease that was recently detected in a number of cattle in the Philadelphia stock-yards, and for which the cattle were condemned, while the dealers were bound over to answer at court for selling diseased meat. This anxious solicitude for the farmers would appear inexplicable were it not for the fact that the Whiskey Trust owns great sheds connected with its distilleries where cattle are fattened on distillery waste for the beef market. It is a matter of common report that the gevernment inspectors at Montreal not very long ago

condemned a considerable number of cattle suffering from antinomycosis, which had been sent to that city from distillery sheds in this country for exportation. It may or it may not be true, but it is reported that this disease is not at all uncommon among cattle in distillery stables. Whether the mode of fattening beef cattle on distillery grains renders them susceptible to this and other diseases, we are not prepared to say, but the subject is of sufficient importance to call for a thorough investigation by the Bureau of Animal Industry."

It would be better to say competent and honest investigators.

THERAPEUTIC NOTES.

(E. W. BING, M.D., CHESTER, PA., TRANSLATOR).

ANTISEPTIC TREATMENT OF PROFUSE DIARRHŒA:

℞.—Bismuth salicylate..... 10 grms.
Sulpho carbolate zinc..... 0 grm. 2.
Chalk mixture,
Distilled water.....āā 50 grms.
Benzoate tr. opium..... 20 grms.
M.—S. A teaspoonful every two hours till diarrhœa ceases.

INJECTION FOR CHRONIC CYSTITIS:

℞.—Crystal carbolic acid..... 1 grm.
Distilled water..... 100 grms.
Dissolve and mix with equal parts of tepid water at time of using.

℞.—Boric acid..... 15 grms.
Glycerine..... 30 grms.
Distilled water..... 300 grms.
S. Dissolve and mix with equal parts of tepid water at time of using.

One or other of these solutions may be employed to wash out the bladder in cases of chronic cystitis. If the mucus has a bad odor, the following injection may be employed:

℞.—Nitrite of amyl..... 5 drops.
Distilled water..... 150 grms.
M.—A dessertspoonful, in a sufficient amount of water, for an injection.

Salicylate acid, 1 to 1,000 parts water, for vaginal injections, is recommended by Prof. Guyon.

Solutions (medicated) for vaginal discharges:

℞.—Tannic acid..... 25 grms.
Water..... 100 grms.

Two tampons, wet with the above, are introduced by aid of speculum, and a third one dry. At end of twenty-four hours they are removed, and detergent injections used and fresh tampons introduced. The tampon cures quicker than injections. Recumbency should be observed and treatment interrupted during menstruation.

Another is as follows.

℞.—Pure carbolic acid..... 1 grm.
Alcohol or cologne water..... 30 grms.
Water..... 70 grms.
Mix.

Use as in previous formula, but renew tampons once or twice a day, following with a slightly astringent injection, then introduce tampons wet with this solution:

℞.—Tannic acid..... 15 grms.
Glycerine, pure..... 90 grms.

Gaz. de Gynécologie.

CONTAGIOUSNESS OF ACUTE TONSILITIS.—Acute tonsilitis is now considered a general disorder from the beginning, infectious, and probably of microbic origin. In certain cases infection is manifested by visceral complications (purulent pleurisy, albuminuria, etc.). The pus of tonsillar abscesses contains staphylococci and streptococci. In acute superficial tonsilitis the intensity of the fever, prostration and tedious convalescence, militate in favor of infection. The author has noted several cases of acute sore throat following in about eight days, the appearance in the waiting-room of the "throat" department of the hospital, of a case of tonsilitis. There had been no previous cases for a long time. These facts seem to prove its contagiousness, although this is not the sole means of the spreading of the disease.

He considers that antiseptic treatment should be instituted and that predisposed persons should be removed from the locality.

—Richardiere, *Rev. de Laryngol. d'Otolog., etc.*

A CASE OF APOPLEXY OF THE UVULA (Le Jeune), — occurred to a lady to whom the author was called in haste, and who was said to have just swallowed during a meal something which had lodged at the upper part of the throat. The patient was very anxious; the voice was hardly changed, and the respiration normal. The sensation of a foreign body declared itself abruptly in swallowing, and since that deglutition had been difficult and painful. The isthmus of the fauces was a normal color, but the uvula was enormous in size. Its extremity was purple in color, of the size of an average olive, and dragging on the base of the tongue; had just been the seat of an active hemorrhage. This lady had not used her voice in singing for some time. She wished, days previously, to recommence singing, and in order to exercise the vocal cords had made fatiguing efforts, producing ultimately the hemorrhage. A puncture by a bistory gave instant relief. The hemorrhage occurred again some moments afterwards, but rapidly ceased, and the uvula returned to its normal size and appearance. —*Rev. de Laryngol., etc.*

FISH DIET AND LEPROSY.—Dr. August Crane, lately Government Physician in Hawaii, writes as follows to a contemporary on the subject of fish diet and leprosy: "From time immemorial the staple food of the native Hawaiians had been fish, and principally raw fish. Until about 1860, there is no history nor tradition of the existence of leprosy among them. At about this date it appeared, and at the time of its appearancce, white people had been among them for forty years, and their food and methods of living had greatly changed. Canned foods, fresh meat and vegetables had been so widely introduced that the use of fish, and especially raw fish, had enormously decreased, and could not be considered as a prominent feature of their diet. At the present day, while consuming no remarkable amount of sea food, one Kanaka out of thirty is a leper. All fish, no leprosy; little fish, much leprosy. This has been the history in Hawaii." These statements above expressed appear to be a formidable indictment against the theory that fish diet and leprosy stand in the relation of cause and effect. But the advocates in favor of the theory are now few and far between, despite the powerful authority of Mr. Jonathan Hutchinson.

—*Hospital Gazette.*

BICYCLE-RIDING is advocated as a remedy for persistent sciatica. It has been successful in cases of two or three years' standing.

The Times and Register

A Weekly Journal of Medicine and Surgery.

WILLIAM F. WAUGH, A.M., M.D., Managing Editor.

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THE SPIRIT OR THE LETTER.

DR. N. R. COLEMAN, of Columbus, Ohio, has forwarded us a pamphlet containing his account of the occurrences that brought about the resignation of his chair in the Columbus Medical College. We have not seen the other side of the controversy, and our remarks will be based solely upon the pamphlet before us. It appears that a student was admitted to the graduating class of the college, whose previous attendance had been in an eclectic college, and his preceptor an eclectic. About the first of February (near the end of the term), Dr. Coleman, as Secretary, desired to notify the student that he was not eligible to graduation, but the Dean prevented the notice being served. On December 24, the Faculty had voted to allow the student to go on with the graduating class, rescinding a contrary motion passed some time previously. The student finished the course, passed his examinations successfully—being, in fact, one of three in a class of fifty-nine who received the grade of 100 in practice of medicine. Nevertheless, the Faculty voted to refuse him his diploma, on the objections stated—*i.e.*, his attendance and study having been with eclectics, and his preceptor having also been an advertising quack. At least, this is the meaning we get from Dr. Coleman's narrative, in which the cases of several students are considered simultaneously.

To this action two members of the Faculty objected, and their protest was carried to the Trustees of the college This body deliberated on the case, and by vote declared the student entitled to his diploma. Dr. Coleman thereupon resigned, and the College Censors passed resolutions sustaining the Faculty and protesting against the action of the Trustees.

Dr. Coleman closes his paper by propounding the following queries:

"1. Has the Board of Trustees of a medical college the right to graduate candidates who have not been recommended by a majority of the Faculty?

"2. Has the Faculty of a regular college the right to graduate men who have studied under, and whose credentials are signed by, those who are ordinarily denominated irregular, or who practice some 'ism' in medicine?

"3. Is it to the best interest, honor and purity of the regular profession of medicine to recommend and urge the graduation of men whose credentials are not up to the full requirement set forth in college catalogues, and in accordance with the ethics of the profession?

"4. Was the position taken by Drs. Hyatt and Pearce, and sustained in the Trustee meeting by Dr. J. W. Hamilton, in urging the graduation of men who were not eligible, according to the published requirements of their own catalogue, for the best interest of the college and the profession at large, and were they justifiable in taking such action?

"5. The Board of Censors consists of medical men who are ripe in years of experience, and who stand in the front rank of their profession. Were they right in their thoughtful, determined, and conscientious act, in condemning, in clear and most emphatic language, the action taken by the Board of Trustees?

"6. The position taken by me in this lamentable affair is purely one of principle and honor; if that position is correct, and for the best interests of the medical profession, let medical men give it their most earnest and sincere approval, and take such measures as their wisdom and judgment may suggest to prevent a recurrence of such an improper and unwarranted exercise of power."

The first question is simply one of law, and appears to be effectually answered by the following quotation from the laws of Ohio:

"SEC. 3726. The Trustees of a college, university, or other institution of learning, incorporated for the purpose of promoting education, religion, morality or the fine arts, which has acquired real or personal property of the value of five thousand dollars, and which has filed in the office of the Secretary of State a schedule of the kind and value of such property, verified by the oaths of the Trustees, may appoint a President, Professors and Tutors, and any other necessary agents and officers, and fix the compensation of each, and may enact such by-laws, not inconsistent with the laws of this State, or of the United States, for the government of the institution and for conducting the affairs of the corporation as they may deem necessary; and may, ON THE RECOMMENDATION OF THE FACULTY, confer all such degrees and honors as are conferred by colleges and universities of the United States, and such others having reference to the course of study and the accomplishments of the student as they may deem proper."

As to the second question, in this particular instance it turns upon the facts of the case. If the student complied with the requirements of the college, he was entitled to compete for its honors.

If the college's law forbids the recognition of time spent in an eclectic college, the student should not have been allowed to enter the graduating class. But, and here is the fatal flaw in Dr. Coleman's argument, when their Dean accepted the student's money and admitted him to the graduating class, the college waived any objections it may have had to his previous course of study. Having once accepted him as a student, and we need scarcely add that the college is responsible for the official acts of its Dean, the

Faculty could not in justice refuse the man his diploma, if he fairly earned it in his examinations.

As to the third question: the medical profession will heartily support any college that will do its share in elevating the standard, by improving its course of instruction, and subjecting its would-be graduates to the most searching tests to determine their moral worth and professional ability. But when the question is one of perpetuating a survival of mediæval intolerance, there is but a small remnant of the profession that will feel any sympathy with it.

The tendency of modern civilization is towards toleration. The days of the Inquisition are past, thank God, never to return. And the downfall of one form of religious denomination does not signify its replacement by another; but the recognition of the personal right of each individual to his own belief. In the Protestant sects, much of the fierce intolerance of the past has disappeared; allied denominations are sinking their differences and coming together on the broad platform of their common belief. It is to be hoped that the true spirit of Christianity flourishes as well under the influence of charity, forbearance and brotherly love, as under the stern rule of the church militant in the "times that tried men's souls." In all the departments of human activity this recognition of personal right seems to be gaining ground, and it cannot be doubted that it is the same in medical circles.

If the Columbus Medical College found the young man possessed of sufficient medical erudition to enable him to pass the examination, the Faculty could afford to disregard his studies in an eclectic college much better than they could overlook a deficiency in his examinations.

When the President of the Philadelphia County Medical Society, in his inaugural address, can openly advocate the admission of homœopathists and eclectics who are not exclusive practitioners of those systems, it is evident that the days of medical sects are numbered.

Annotation.

PHILOTHION.

WE have just received from Dr. J. de Rey-Pailharde, Civil and Mining Engineer, of Toulousse, a very interesting report of his experimental researches into the properties of an agent named by him philothion, a special hydrogenous matter, which exists in both the animal and vegetable kingdoms, and which probably plays an important part in the absorption of oxygen by using cells. While studying the mechanism of the passage of free sulphur into the circulation, when taken into the stomach, he satisfied himself that the hydrogen, evolved by the process of fermentation, combines with sulphur, and produces sulphuretted hydrogen. Later on he demonstrated that both the yeast of beer and living cells contain an organic matter that produces sulphuretted hydrogen in the presence of sulphur. He then tried to isolate the cells that contained this matter without success; but finally, after many failures, he ascertained that beer yeast yields it under the influence of strong alcohol and pressure, and the product responded to the same tests that had been tried in the case of philothion contained in the animal tissues; that philothion combines at ordinary temperature with sulphur, oxygen, carmine of indigo, and tincture of sunflower; that it forms H^2S with sulphur, and probably H^2O with oxygen; that the cells consuming the largest proportion of free oxygen are those holding the most philothion; that philothion is not destroyed, in spite of the incessant influence of oxygen in the living cells, and that oxygenation is produced in the close relationship of the tissues, since philothion exists in the midst of their anatomical elements.

The abundance of philothion in tissues that consume much oxygen shows that it plays a special rôle in the absorption of free oxygen by the cells.

In the tissues of animals recently slaughtered, as oxen, horses, sheep, lambs, dogs, rabbits, hares, etc., he has found philothion in the muscles, brain, kidneys, spleen, testicles, liver and pancreas, but little or none in the bones, blood, lung-tissues, or fat. Philothion is also found abundantly in vegetable tissues, particularly in the young and active parts, which are those that consume the most oxygen.

Dr. de Rey-Pailharde admits that these experimental studies have not been exhaustive, but he intends to prosecute them still more vigorously in order to elucidate the chemical and physiological properties of philothion. It seems certainly proved, so far, that it absorbs oxygen, just like leucomaines, soluble ferments, glucose in alkaline solution, etc., and that it forms sulphuretted hydrogen when it comes in contact with sulphur that has been taken into the stomach.

Letters to the Editor.

WHAT IS IT?

MAN, aged forty-five years; widower; merchant by occupation; stout and rather plethoric, with good family history; was taken about five months since with a severe attack of sciatica of right leg, which persisted for about one month, but was immediately followed by an obstinate and severe attack of lumbago, very localized at times, with at one time coccygodynia. Since that time more or less soreness and stiffness of the muscles in different parts of the body (neck, back, chest), with occasional sciatic pains on both sides, have continued. For a few weeks prior to the beginning of this trouble it was noticed that the patient was getting pale and somewhat reduced, and the pallor and general debility of the case has persistently increased, notwithstanding the fact that his appetite has been well preserved, and has taken large quantities of nourishing diet. A marked nervous condition (almost hysterical at times) has been a prominent feature of the case a greater portion of the time. A varying and irregular fever has also continued most of the time, and has run about like the enclosed chart, which covers a period of about one month. The pulse has also been more or less accelerated, and of late shows signs of heart failure by reason of weakness and intermittency.

There has been an almost continual loss of flesh, flabby muscles, dry skin and corresponding loss of muscular strength. I have observed that the pulse rate and temperature are apt to be raised during these attacks of pain, which are more likely to be present during or before threatening or stormy weather.

A venous neck hum and an anæmic murmur are well marked. Urine loaded with urates and phosphates.

Quantity and specific gravity varying inside of health limits. No dropsy. No hemorrhages. No cough. Symptoms generally negative, excepting the pinched and shrunken features; the marked and persistent anæmia; the great muscular debility, and the recurring pains.

The treatment of the case has been mainly antirheumatic and supporting. Salicylate, salol, phen acetine, and iodides in combination with colchicum, etc., has been tried in turn in conjunction with juice and meat liquors. The various forms of iron, arsenic, strychnine, etc., with maltine, with cod-liver oil, and the hypophosphites were given as tonic measures. None of the above remedies have seemed to be of any service or benefit to the case, and, if possible from the above brief sketch, I would be pleased to have your full opinion as to what might be the difficulty in this case, together with any therapeutic suggestion which will help to relieve both my patient and myself from this discouraging case.

I think the special numbers which you are issuing occasionally are taking well, and I hope they will be continued.

DR. B. F. SHARPLESS.

CATAWISSA, PA.

ACROMEGALY?

FOR nearly a year I have had under my care a very wealthy gentleman, who in the beginning of his illness developed la grippe. He recovered from this, but a harassing cough continued to trouble him; also obstinate constipation. For the former I used Freligh's tablets, saw palmetto, and host of remedies, of which I might again give malto yerbine, etc. He has not entirely recovered from this trouble, though I have discontinued treatment for it, as he has tired from taking medicine. He has a regular heart, though passes urine a dozen times per day, and is obliged to get up from three to four in the morning to urinate. He is very stout; weighs two hundred and seventy-five pounds, and for the past fifteen or twenty years has been a very heavy user of whiskey, though his tongue keeps clean, sleeps quite well, and has a good appetite. He shows evidences of a progressive neurosis, such as a failing of the muscles of the right arm, and an inability to use his legs; if he does, complains of a weak condition, and not able to walk any distance.

His mind is good. He will soon be sixty-nine years of age. For his constipation I have used almost every thing, and each new remedy soon loses its effect. It does seem that nothing can be done to restore his vitality, as nothing seems effective for any length of time. He has not been out of the house for three months through fear he may contract a cold.

His immense proportions make his exercise very little; in fact, the only exercise he takes now is going up and down two flights of stairs three times per day for his meals, the rest of the time he sits in his room. There are so many symptoms that I have not enumerated, but have given you the most formidable ones. What I am anxious to obtain is a remedy to keep his bowels regular, and yet be agreeable. As to being able to give him new life and activity, I am sure I can see nothing that can be employed to do any good, and may add he has taken phosphide of zinc, and bichloride of mercury, and is now taking hunyadi water, supported by rectal injections, to regulate his bowels. Would be pleased for your opinion and suggestion for treatment.

C. W. HUGHES, M.D.

BIG RUN, PA.

[Possibly a case of acromegaly. For the bowels nothing is better than malt extract with cascara sagrada, aided by enemas of saturated salt solution, ice cold. The laxative pill generally answers well.]

Book Notices.

THE ELECTRO-THERAPEUTICS OF GYNÆGOLOGY. By AUGUSTIN H. GŒLET, M.D. Parts I and II, with illustrations. 12mo, pp. 186–194, 196–203. Geo. S. Davis, Detroit, 1892.

The distinguished author has succeeded in getting out a work of great value to the practitioner, owing to the clearness of his style, to the new information given, and to the thoroughness of his reasoning. That electricity is now getting hold in such cases as have been spoken of by specialists for a long time past is true, for the value of this form of relief and cure cannot any longer be doubted by the gynæcologist (unless he is one of the rabid laparotomists who see nothing but castration in all that is handled by him), and many a woman is living to-day a monument to the conservatism which demands medical in lieu of surgical handling in many instances. The book is elegantly gotten up, and will be very serviceable to those who employ electricity in their work.

—W. R. D. B.

The Medical Digest.

HYDATID OF THE LUNG.—In all cases of suspected hydatid of the lung, a long, very fine aspirating trocar and needle should be used for confirmation of the diagnosis and gauging the exact distance from the surface of the chest to the most superficial part of the tumor. The exploring needle should be much longer, but not much larger than an ordinary hypodermic needle. If a larger needle were used there was danger of swamping the thoracic cavity with fluid, and causing suffocation or fatal syncope. Serious symptoms never follow the aspiration of a hydatid tumor in the thorax. If the hydatid were very small and situated in the substance of the lung, it might be treated by simple aspiration, the removal of a small quantity of fluid from the cyst often causing it to wither up and undergo degeneration. If the tumor were large, and especially if it contained daughter cysts, if suppuration had taken place, or if the fluid after tapping reaccumulated, it was necessary to make a deliberate incision over the most prominent and surgically convenient part of the tumor, and completely remove the hydatid with its mother cyst. If the tumor were very large it might be necessary to remove a portion of one, two, or even three ribs, in order to completely evacuate the hydatid, and provide for drainage.

—Dr. Maunsell, *Clin. Soc.*, *London Lancet.*

OBSERVATION ON EUROPHENE.[1] — Although, in view of the short time that has elapsed since the introduction of europhen, the number of cases treated is too small to warrant positive deduc-

[1] Reprinted from the *Balneologishe Centralblatt*, II Jahrgasg. No. 13. By Dr. W. H. Gilbert, House Physician of the Sanatorium at Baded-Baden.

tions, the utility of the remedy is rendered probable by the observations of many authors, and further experiments are therefore called for. Up to this time I have treated seven cases with europhen, comprising two cases of ulcer of the leg, one case of chancroid, scrofuloderma, and burns of the second degree, and two cases of recent wounds with loss of substance.

The two cases of ulcers of the leg presented the most unfavorable conditions for treatment. The patients were females belonging to the laboring class, and were respectively sixty-nine and fifty-five years of age. In consequence of their occupation they had been unable to take the proper amount of care, and hence ulcers had formed over the marked varicositis, which persisted for many years, despite the employment of remedies of all kinds. In the case of the older patient, Mrs. B., the entire left leg and foot were the seat of a firm infiltration and eczema. On the middle surface of the lower third of the leg, there was present an ulcer about as large as the palm of the hand and covered with flabby, torpid granulations, which had resisted various methods of treatment and was constantly spreading.

The younger patient, Mrs. F., had on the inner aspect of the middle third of the right leg an ulcer about 4 centimeters in length and 3 centimeters in breadth, which had originated from a number of varicose rodes, and was surrounded completely by inflammatory induration. I ordered the following treatment: In the evening a tepid bath of chomomile flowers, then insufflation of the entire surface with europhen, application of a loose dressing of gauze and cotton, and of a muslin bandage. In the morning a 2 per cent. of ointment of europhen, with vaseline and lanolin (equal parts) was applied, and the parts dressed and bandaged as before. The chamomile baths and the alternate use of europhen in the form of a powder and ointment, had the effect of preventing the crust formations. Under this treatment, vigorous granulation tissue soon developed, and at the end of about three weeks the ulcers had cicatrized, and healing has remained permanent up to the present time. These cases are opposed to the statement of Rosenthal that europhen is valueless in ulcers of the leg.

In a case of chancroid I obtained extremely prompt healing by means of europhen. The patient consulted me for a large ulcer, which had spread over the entire upper surface of the glans penis, and had produced an extensive loss of substance. I employed the method described by Nolda, which is as follows:

Every morning and evening the ulcer is thoroughly irrigated with sublimate solution, dried with cotton, and insufflated with europhen. At the end of ten days the patient was discharged cured.

Europhen was employed also in a case of scrofuloderma occurring in a girl thirteen years of age. The patient had the erethic habitus, and besides the other symptoms of a severe scrofulosis showed on the neck, the nucha, the angles of the lower jaw, the forearms and legs, numerous deep ulcerations, which for the most part took their origin from the lymphatic glands. The long-continued employment of such remedies as iodoform, balsam of Peru, and nitrate of silver had had no effect in diminishing the torpidity of the ulcers. I then adopted the following local treatment:

The abscesses were opened and the ulcers incised wherever it seemed indicated. Europhen was then applied in the form of the powder, and of a 2½ per cent. ointment, with vaseline and lanolin (equal parts), alternatively morning and night. Every third evening a dressing consisting of wads of cotton soaked in a 2 per cent. solution of carbolic acid was applied and allowed to remain all night. These procedures were always proceeded by careful antiseptic cleansing and drying of the parts. To improve the patient's general condition, I also ordered arsenic and a proper diet.

After this treatment had been continued for a number of weeks the ulcers cleared up, vigorous granulations developed aed citratrization occurred, while the tendency to formations of new ulcers seemed to have been diminished.

At any rate, by this method of treatment, I obtained results which previously could not be attained in any other way, and by persisting with it, I hope to effect ultimately a comparative cure.

The case of trauma due to thermal influences which I treated with europhen, was one of the second degree. A servant girl, while engaged in washing, was scalded over the entire left forearm by a large quantity of boiling water, so that an extensive formation of bullæ took place. After careful evacuation of the blabs, I applied a dressing of the europhen ointment described above, which was renewed daily, and under which healing ensued promptly in the course of a week.

My further experience with this remedy relates, as above mentioned, to two cases of fresh wounds.

A young girl, while cutting bread, injured the first phalanx of the left index finger, a considerable defect of the skin and soft parts, resulting in consequence of the oblique position in which the knife was held.

Under the alternative use of europhen powder and ointment the wound secreted profusely, then became cleansed, and converted into a healthy granulation surface, which rapidly cicatrized from the margins.

The second case was that of a man, who in a scuffle receive a blow upon the top of the head with the metallic knob of a cane. Two V-shaped converging wounds resulted, about 3 centimeters in length, with jagged margins, and marked contusions of the surrounding parts. The wounds extended to the periosteum, which was also injured. The scuffle occurred at night, and the patient consulted the nearest barber, who shaved off the hair over the injured area and applied a strip of adhesive plaster.

Thanks to the "antiseptic" properties of the plaster, the patient came to me on the following evening with violent pains and inflammatory manifestations, swelling of the lymphatics, and marked suppuration of the wounds, the entire strip of soft parts between the sequent having been destroyed by the process. A few incisions were found necessary to obtain the recesses which had formed between the soft parts and cranial bones. After irrigation with sublimate solutions I sutured the wounds and applied a europhen gauze dressing. In this case I proceeded as in the others, by the alternative application of europhen powder and ointment, after previous irrigation and sponging of the parts.

The wounds healed remarkably well by second intention, the V-shaped defect of the soft parts filling up with healthy granulations and cicatrizing with astonishing rapidity.

As far as our present experience extends, it may be positively affirmed that europhen is preferable to iodoform in many particulars. Whether the remedy possesses poisonous properties as yet undiscovered, which would render its use associated with danger, must be left to time and empiricism to determine. This assumption is, however, in no way justified in the light of our present knowledge, and the favorable

results thus far obtained certainly warrant us in taking a sanguine view of the remedy and encouraging the use of europhen.

In conclusion, I would again call attention to my method of alternative applications of europhen in the form of powder and ointment, by which crust formations are prevented.

WHERE ARE THE HEART SOUNDS PRODUCED.—This question, often asked, has been answered by at least twenty different explanations. That of Rouanet, who refers the first sound to the tension and shutting of the large valves, and the second to the tension of the sigmoid valves, is the one which has the greatest number of partisans, and is also the one which, alone, has been considered correct.

There is not entire accord on one point, however—are the heart sounds produced entirely by a single sound, or by several, formed simultaneously in different places? This point is of secondary importance, as the closing and tension of the valves are everywhere regarded as the predominating causes in the promotion of the sounds; beyond that, whatever contributed to their constitution counts for nothing.

Experiment shows, that membranes rendered suddenly tense by fluids (gaseous or liquid), produce sound, and, moreover, the intensity and other qualities of this sound depends, more or less, on the thickness of the membranes, their degree of tension, and greater or less mobility. This dependence can be generally expressed in the following manner: The more the membranes are equal in thickness, the greater their tension and mobility, so much the more easily can they produce sounds. Fuller details might be given, but they would be useless. In fact, on the other hand, the structure of the tri and bi-cuspid valves, their tension, and manner of closing are so complicated, that the most explicit details could not bring us to surer conclusions, than those which can be deduced from the explanations already given, and, on the other hand, for the sigmoid valves, such detail would be superfluous. After this short digression we shall see if the already mentioned experiment will enable us to arrive at a decision as to whether the heart sounds are formed where general opinion supposes they are. To this end we wish to give a description of the tri and bi-cuspid nerves. These valves are formed of elastic membranes cut out into two or several leaves of different size; these leaves or valves are also of an equal thickness, and this inequality is due to the manner of insertion of the tendinous cord, that is to say, the tendinous cords, with their dichotomous divisions, are implanted on the leaves at different distances and at different spots on their faces; as well on their centers, as on their edges, and on their periphery, and at all these places thickening is produced, whilst the internals preserve their natural thickness (or rather, thinness). The valves themselves are by their peripheral part fixed to the party wall of the heart, between the auricles and ventricles, but they are free for the rest of their extent. This free zone is, however, limited by the tendinous cord just spoken of.

The closure of the valve is produced in the following way: The free borders are strongly folded back against the ventricular cavities and engage with each other, whilst their other parts are bent into rounded projections or pressed so strongly against each other that they form folds which are turned as much towards the auricles as towards the ventricles. The movement of the valves towards their meeting point is of a varying rapidity for different valves. To review, we find valves of different size, membranes of unequal thickness and tension, folds and compressions, none possessing complete mobility. On the contrary, this mobility is hindered at nearly all points on the faces of the valves. In spite of that, there has been no hesitation in saying that the first sound is caused by these valves. That is a conclusion which is certainly not well founded, and which should be rejected. If the first sound is not produced by the valves, where then is it formed? In the chordæ tendineæ, perhaps? Let us examine this. The chordæ tendineæ consist of slender, elastic cords, which, during the closure of the valves, are stretched in two opposite directions toward the auricles and toward the ventricles. Toward the former they are stitched by the valves to which they are attached, and whose movements they follow; towards the latter their tension is due to the musculi papillarii. Here is a strong analogy to what is seen when fine threads are taken between two fingers of each hand and carried rapidly by them in contrary directions. The general rule in this case is that sound is produced. Why, then, should not the tendinous cords equally produce by their tension a sound? The answer would seem to be in the affirmative, but we must not conclude too hastily, for there exists in the tendinous cords certain anatomical details which render the production of sound a very difficult matter. In the first place, their fibers are short, which, for the formation of sound, would require of them a stronger tension, than what, in all probability, they are subject to, for the purpose of closing the valves. In the second place, tendinous cords are so divided at their extremities, and so much exposed to unequal tension by their numerous insertions, that it is probable that they could not make sufficiently regular vibrations to produce a sound. For this reason it is supposed that the action of the chordæ tendineæ cannot be the seat of the first sound.

An experiment, differing from those already mentioned, teaches us, that when a fluid is projected from a reservoir through a small aperture by a pressure greater than that of the surrounding medium, with a flow, slightly contracted at the level of this orifice, it forms a sound; ordinary wind instruments may be mentioned as examples.

Now, if in the heart an analogous arrangement is found, may we not conclude that that may be the cause of the sounds?

The fact is, that a similar arrangement is found in the heart; we meet with at first, two large reservoirs, the ventricles in these reservoirs are two apertures of smaller size, the pulmonary and aortic orifices, and finally, in these vessels, "tongues," formed of fine membrane homogeneous, elastic, always slightly tense, possessed of all the requirements necessary to the formation of sound. Further, across these openings and "tongues," there passes, from time to time, a current of greater pressure than that in the blood-vessels. Is there not, in all this, a complete analogy with what is seen in instruments? Why, then, should not the blood current, passing through the orifices of the pulmonary artery and aorta, throwing back more or less completely the "tongues" (of the valves), put these into sufficiently strong and regular vibrations to produce sound? Because the fluid is a liquid in place of air? But if we remark that the laws for the production of sound with liquids, are in general the same as those for its production with air, what other objection is to be made? None, as it seems to me. If the analogy to the construction of instruments is as complete as it appears

to be, would it not be a misconception to deny the formation of sound in these parts? Therefore, the conclusion would seem to be that the first sound is formed by the sigmoid valves.

The formation of the second sound is due, as generally admitted, to the sudden tension to which the sigmoid valves are subjected, at the commencement of the diastole, by the back pressure of the blood current.

By the diastole, there is produced in the ventricles, a sort of negative pressure which gives the pressure in the arteries a considerable superiority by which the valves are suddenly and easily stretched, and this tension should bring about a sound. This assertion easily finds its justification in the first cited experiment. From what has been stated, the first as well as the second sound should be formed by the sigmoid valves, the first sound during the contraction and the second during the dilatation of the muscles, from the back pressure of the arteries. These are theoretical didactics, capable of confirmation by experimental demonstrations.

Some years ago, I constructed an apparatus by means of which could be reproduced in the bullock's heart the action of the valves and the heart sounds. By the aid of this, the following experiments were made:

In the first place, in the bullock's heart the play of the valves and the sounds were proved to be perfectly normal after a careful examination. Then the tricuspid valve was sounded, and auscultation of the two sounds could be made as well in the right as in the left heart—the suppression of the tricuspid had not caused the disappearance of the first sound in the right heart.

Next, the sigmoid valves of the same heart, were removed from the aorta. The first and second sound could be reproduced in the right heart, where the sigmoid valves were perfect, but the tricuspid removed, whilst on the left heart no sound could be heard. The suppression of the aortic valves had suppressed both sounds in the left heart, and the mitral valve was not in condition to produce any sound.

In a second heart both the mitral and bicuspid valves were removed, but the sigmoid valves left intact. In this case both sounds were produced as well in the right heart as in the left. The suppression of the large valves had not suppressed the sounds.

In a third heart the sigmoid valves were removed, but the cuspid valves left intact; both sounds were suppressed. These experiments allow only a single conclusion—that the sounds are produced by the sigmoid valves.

Experiment thus supports theoretical reasons. Finally, to prove that it cannot be the shock of the valves against each other which produces the second sound, in one of the hearts the leaflets of the aortic and pulmonary valves were removed, and where there could be no question of the valves striking each other the characteristic second sound was produced.

—Dr. Sandborg, *Le Progres Medical.*

Medical News and Miscellany.

DARWIN is erroneously supposed by some persons to have suggested that men were descended from apes. But he only adduced arguments in support of his opinion that a common ancestor produced both men and "anthropoid" (or "men-like") apes. And the Lord knows there are plenty of men like apes.

AN Italian duchess, having given birth to a boy about the time her husband returned from a two years' absence, is trying to convince the court that the child is the product of union with her husband through the medium of his ghost. She has not yet stated whose material person the Linga assumed on that occasion, and that is just what the duke is most anxious to ascertain.

THE Gross Midwifery Dispensary, of Denver, Col., opened its service May 1, 1892. The staff consists of: Consulting Obstetricians, Dr. Amasa M. Bucknum and Dr. Thomas H. Hawkins; Attending Obstetricians, Dr. William H. Buchtell, 1616 Glenarm street; Dr. Charles B. Richmond, 1616 Glenarm street; Dr. Charles B. Schollenberger, 2150 Larimer street, and Dr. Carey K. Fleming, California Building, Seventeenth street.

WEEKLY Report of Interments in Philadelphia, from April 30 to May 7, 1892:

CAUSES OF DEATH.	Adults.	Minors.	CAUSES OF DEATH.	Adults.	Minors.
Alcoholism	1		Indigestion		1
Apoplexy	12		Inanition	2	12
Bright's disease	9	2	Influenza	2	
Burns and scalds	11	3	Inflammation, brain	2	7
Cancer	9		" bronchi	3	5
Casualties	8	2	" kidneys	7	1
Cerebro-spinal meningitis	1	2	" larynx	1	
Congestion of the brain	4	9	" heart	1	2
" " lungs	1	2	" lungs	26	22
" " liver		1	" peritoneum	3	2
Congestive chill	1		" s. & bowels	8	13
Cholera infantum		1	" trachea		1
" morbus	1		Insanity	3	
Cirrhosis of the liver	1		Jaundice	1	
Child birth	1		Marasmus		16
Consumption of the lungs	42	4	Measles		5
Convulsions		19	Œdema of lungs	1	
Chicken pox		1	Obstruction of the bowels		1
Croup		7	Old age	14	
Cyanosis		4	Paralysis	5	
Debility	5	3	Purpura hemorrhagica		1
Diabetes	1		Rheumatism	2	
Diarrhœa		1	Scrofula		2
Diphtheria	1	19	Septicæmia	5	1
Disease of the heart	21	5	Softening of the brain	2	
" " liver	2		Shock, surgical	1	1
Dropsy	3		Spina bifida		1
Effusion of the brain		1	Suffocation, illuminating gas	1	
Embolism, cerebral	1		Suicide	2	
Erysipelas	1	1	Syphilis		1
Enlargement of the heart	3		Tetanus		1
Fatty degeneration of the heart	2		Tumor	5	
Fever, puerperal	2		Ulceration of the bowels	1	
" scarlet		11	" " stomach	1	
" typhoid	5	3	Uræmia	2	
Fracture of femur	1		Whooping cough		3
Goitre, exopthalmic	1				
Gangrene, senile	1		Total	253	201
Hemorrhage		1			

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